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" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

———————

—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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ERRATA in Messrs. Stoney and Reynolds's Paper on the Cause of the Interrupted Spectra of Gases, Phil. Mag. July 1871.

Page 47, line 2, *for* $(\theta + \frac{1}{5}\pi)$ *read* $(\theta + \frac{1}{5} \cdot 2\pi)$.
,, — ,, 3, *for* $(\theta + \frac{2}{5}\pi)$ *read* $(\theta + \frac{2}{5} \cdot 2\pi)$.
,, — ,, 4, *for* $(\theta + \frac{3}{5}\pi)$ *read* $(\theta + \frac{3}{5} \cdot 2\pi)$.
,, — ,, 5, *for* $(\theta + \frac{4}{5}\pi)$ *read* $(\theta + \frac{4}{5} \cdot 2\pi)$.
,, — ,, 16, *for* form (5) *read* form (7).

I. *On the Universal Powers of Nature and their Mutual Dependence. By* L. A. COLDING*.

MY previous works upon this subject have been so favourably accepted by the Royal Scientific Society, that I am encouraged once more to lay before the Society some inquiries founded upon the principle of the Lost Forces, which I formerly stated; and I am the more happy at having been requested to continue my researches, which have afforded me many pleasant recreations from my other occupations, because the sequel will contain a basis for a series of inquiries which, I am sure, are in many respects not without interest.

On former occasions, as is well known, I have in part referred to the intimate connexion which is proved to exist between the powers of nature; in part I have tried to explain the common law according to which the respective powers of nature may be developed from each other; and the correctness of the fundamental principles proposed here has been confirmed by experiments, which I have performed upon the heat developed by friction of solid bodies.

I cannot omit the remark that just as it is the various forces of nature connected with the parts of matter which continually have caused and continually will cause the incessant development of the endless variety of different bodies which nature presents at all times, and just as the peculiar character of the bodies is owing to these forces, so the incessant change which, in fact, may be considered to be the characteristic of matter is caused by their mutual effect. But a general view of the

* Communicated by Professor Tait.

various energies must evidently call forth the idea that they also
are produced and developed for the purpose of disappearing after
having performed one or another effect on the particles of mat-
ter; for, in the first place, it is well known that every kind of
energy (as, for example, energy of heat, mechanical energy,
electric energy, &c.) is able to produce all these energies; and,
secondly, we know that when quantities of mechanical work,
quantities of heat, &c. are produced through certain quantities
of work, quantities of heat, &c., then these energies disappear
by degrees as new ones are produced. It is a well-known fact,
too, that the production of heat through heat, or of quantities
of mechanical work through quantities of mechanical work, &c.,
is in reality nothing but the energy imparted from one system of
material particles to another, and no new production, and also
that the receiving body can at most merely receive an increment
of energy of the same quantity as that which the imparting body
loses; on the contrary, keeping to the indistinct view of the
energies having acted their parts when certain material results
have been produced, we have not as yet formed a clear idea of
the general proportion between the acting and the producing
forces. Thus, for example, when the mechanical energy con-
tained in a quantity of water falling upon a water-wheel drives a
saw-mill, then it produces every moment a certain material
result, but the corresponding mechanical energy itself is lost.
Or when heat, developed by burning coal under a steam-boiler,
moves a corn-mill by means of a steam-engine, then the heat
likewise every moment produces a material result, at which the
common idea stops; but the energy of heat which has produced
this result exists no more, and we say it has become latent.   In
the same manner, when the electrical current developed by che-
mical forces is performing a certain work by means of an electro-
magnetic machine, then its energy disappears during the work,
&c.   That new forces, as heat, electricity, &c., are developed
along with the material work is indeed well known; but this is
generally considered a secondary thing. This view has, how-
ever, always appeared to me a very unpleasant one; and I
think, on the contrary, that the only natural view of this subject
is, as I explained before: *That the forces by no means vanish in
matter, and consequently it must be a general law of nature that
the forces, without exception, undergo a mere change when they
seem to vanish, and afterwards reappear as active sources of power
of the same quantity but under different forms*[*].

If the alleged proposition is correct, it is evident *that the*

* See *Die organische Bewegung in ihrem Zusammenhange mit dem
Stoffwechsel*, by Dr. J. R. Mayer (Heilbronn, 1845); and Helmholtz, *Ueber
die Erhaltung der Kraft* (Berlin, 1847).

different kinds of energy, as energy of heat, mechanical energy, the energy produced by the mutual effect of chemical forces, &c., cannot be of different essence, but that all the various kinds must be looked upon as one and the same energy, as, for example, mechanical energy.

As heat consists in the motion of the particles of bodies, it follows from this:

1. *That the material particles of which bodies consist are in continual motion, even when the particles of the body seem to be at perfect rest;* and .

2. *That, in investigations into the internal motions to which the bodies are subject, we need not look upon heat as a particular force, but rather as the result of the existing attractions and repulsions in connexion with certain quantities of motion imparted to the particles of the body.*

As to the condition in which the material particles of a body are, it is most natural to admit, with Davy, that, according to their nature, the smallest elementary particles of the body possess a certain electric force, through which they attract or repel the other material particles of the body. The proportions between the quantities of the various elements contained in the body, as well as the number of the different elements and the quantity of their electric forces, determine the positions of equilibrium of the individual particles as depending upon the adjacent particles and their internal groupings in bodies. About these positions of equilibrium, which for each individual particle are determined by the attractions and repulsions of all the other particles, the particles attracted and repelled continually vibrate on account of the imparted momentum ; and, in my opinion, the heat of the body consists in this motion, which, like any other kind of motion, may be more or less, according to circumstances. Thus it is obvious that the internal quantity of energy in a body will increase as well when a new quantity of energy, whether in the shape of mechanical energy, or of heat, electricity, &c., is imparted from one body to another, as when those forces are increased by which the particles of the body are moved among themselves. On the contrary, the quantity of energy will decrease when some part of the motion contained in the body is conducted into other bodies, or when the forces decrease by which the particles of the body are moved.

When no energy is imparted to or taken away from the body, and the forces by which the particles of the body are moved among themselves do not change, then the energy contained in the body will remain always the same.

Now the problem is to determine the proper mathematical expressions for the energy contained in a body. In consequence of

the preceding remarks this will not be difficult, as we have seen
that the various kinds of energy are in reality not different, but
that all of them may be considered as one—for example, as me-
chanical energy.

As we consequently have to determine the general mathema-
tical expression for the mechanical energy among the material
particles, or, which is the same thing, to determine the mathe-
matical expression for the total integral of *vis viva* which has
been called forth among these particles by an originally existing
source of motion, the following well-known example may be
useful.

If a quantity of water m be at rest at the height h above the
surface of the earth, and if h is small enough for us to suppose
the force of gravity at the height h to be equal to the force of
gravity g at the surface of the earth, then it is a truth admitted
by all, and completely proved, that the whole integral of motion
which may be produced and imparted (for example, to a water-
wheel, or to any other machine) through the force of gravity will
be expressed by

$$Q = m \cdot g \cdot h;$$

which effect, however, we shall only be able more and more nearly
to approach, never to obtain entirely, on account of the impedi-
ments which always occur, such as resistance of air, resistance of
friction, &c.   As $m \cdot g$ is the weight of the water, and h is the
height through which the water is allowed to fall, we perceive that
if $m \cdot g$ is expressed in pounds and h in feet, then the mechanical
energy, which in mechanics generally is called the quantity of
work, is to be expressed in foot-pounds—that is say, in pounds
raised one foot.

Further, it is well known and proved that, if we abstract from all
those resistances which in fact will occur, then we obtain exactly
the same quantity of work, whether the water moves vertically in
the direction of gravity or is forced to move along any inclined
plane or any curved line through the height h ; the consequence
of which is that the increment of quantity of work dQ which is
developed by the falling through each little part ds of the path s
is equal to $m \cdot g$ multiplied by ds, resolved in the direction of the
force ; that is,

$$dQ = m \cdot g \cdot \frac{dh}{ds} \cdot ds.$$

But it may easily be perceived that this formula would hold true in
general, even if the accelerating force g were any variable quan-
tity g' and m any mass, as g' will always remain constant during
the element of time dt in which the element of path ds is de-
scribed.   If, then, we put the accelerating force resolved in the

direction of the path

$$g' \cdot \frac{dh}{ds} = \phi,$$

and the velocity in the path, after the lapse of time t, is put equal to v, then we have the increment of energy expressed in general by

$$dQ = m \cdot \phi \cdot ds = m \cdot \phi \cdot vdt.$$

We also perceive that the unit of this quantity Q is still the same as observed before, viz. one pound raised one foot.

But if the rectangular coordinates of the material point are denoted by x, y, z, and the accelerating forces in the direction of the three coordinate axes by X, Y, Z, then we have

$$\phi = X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds},$$

which, substituted in the above equation, gives

$$dQ = m\left(X\frac{dx}{ds} + Y\frac{dy}{ds} + Z\frac{dz}{ds}\right)\frac{ds}{dt} \cdot dt, \quad . \quad . \quad (1)$$

from which the energy produced during the time t is found, viz.

$$Q = m\int (Xdx + Ydy + Zdz) + C_1, \quad . \quad . \quad . \quad (2)$$

C_1 being an arbitrary constant.

If, on the contrary, the point is not perfectly free, but subject to any material resistance, such as the resistance of a fluid, resistance of friction, &c., then the increment of energy during the time dt will only be

$$dw = m\left(\frac{d^2x}{dt^2} \cdot \frac{dx}{ds} + \frac{d^2y}{dt^2} \cdot \frac{dy}{ds} + \frac{d^2z}{dt^2} \cdot \frac{dz}{ds}\right)\frac{ds}{dt} \cdot dt, \quad . \quad (3)$$

from which we find the energy, which in fact is contained in the point after the lapse of time t, viz.

$$w = m \cdot \frac{v^2}{2} + C_2, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where C_2 is an arbitrary constant.

The measure for this energy is still, as before, 1 pound raised 1 foot, which is easily ascertained by observing that the quantity of energy w might also have been obtained by causing the mass m to fall through a height h of vacuum so great that the terminal velocity thereby had been v, which depth of fall is determined from the equation

$$\frac{v^2}{2} = g \cdot h, \text{ as } g \text{ is the force of gravity.}$$

If this value for $\frac{v^2}{2}$ is substituted in the expression for w, formula (4), then we get w expressed simply in foot-pounds.

The energy which the material point loses during the motion in the time dt may consequently be represented by

$$dq = dQ - dw.$$

But this energy is only apparently lost when it seems to vanish; for, so far as the stated principle is correct, it reappears in its original magnitude merely under another form.   Thus the new energy may be represented by

$$dq = m\left[\left(X - \frac{d^2x}{dt^2}\right)\frac{dx}{ds} + \left(Y - \frac{d^2y}{dt^2}\right)\frac{dy}{ds} + \left(Z - \frac{d^2z}{dt^2}\right)\frac{dz}{ds}\right]\frac{ds}{dt}.dt. \quad (5)$$

This equation, which may easily be put into the form

$$dq = m\left(X - \frac{d^2x}{dt^2}\right)\frac{dx}{dt}dt + m\left(Y - \frac{d^2y}{dt^2}\right)\frac{dy}{dt}dt + m\left(Z - \frac{d^2z}{dt^2}\right)\frac{dz}{dt}dt,$$

shows, to begin with, that we get the whole new energy by taking the amounts of the energies which the accelerating forces in the direction of the axes will separately produce.

If we imagine the material point to be subject to any kind of resistances, and if we denote the resultant of them all by R, then this may be supposed to be resolved into two others—viz. into the resistance in the direction of the path (which I shall denote by P), and into the resistance perpendicular to the path (which I shall call P_l).   We have then, as known,

$$P = m\left[\left(X - \frac{d^2x}{dt^2}\right)\frac{dx}{ds} + \left(Y - \frac{d^2y}{dt^2}\right)\frac{dy}{ds} + \left(Z - \frac{d^2z}{dt^2}\right)\frac{dz}{ds}\right],$$

which, substituted in the equation (5), gives

$$dq = P\left(\frac{ds}{dt}\right)dt,$$

from which we get by integration,

$$q = \int P . \frac{ds}{dt} . dt + C, \quad . \quad . \quad . \quad . \quad . \quad (6)$$

in which C is an arbitrary constant.

Hence follows *that the newly produced energy depends only on P, or the resistance in the direction of the path, whereas it is independent of P_l, or the resistance perpendicular to the path*[*].

* The last result may perhaps not appear to be essentially different from what is immediately derived from formula (1), if we only look upon the material resistances in the directions of the three coordinate axes as real forces, which we might suppose to be included in the accelerating forces X, Y, and Z; but partly it is obvious that formula (1) would then represent the increment of the quantity of energy which the moveable body in fact would receive during the time t, consequently what is expressed in formula

When $X dx + Y dy + Z dz$ is an exact differential which we may denote by $d \cdot F(x, y, z)$, then formula (5) by integration simply gives

$$q = m \cdot F(x, y, z) - \frac{m}{2} \cdot v^2 + C. \quad . \quad . \quad . \quad (7)$$

As a particular case, I will here regard that in which the resistance P in the direction of the path is the constant, as in Coulomb's experiments with friction of metal sliding on metal; agreeably to formula (6) we then get

$$q = P \cdot s,$$

when q is supposed $=$ zero for $s = 0$.

This equation shows that the newly developed energy is equal to the product of the friction and the space passed through, which is in accordance with my earlier experiments, and that the amount of this energy is independent of the velocity with which the slider is moved; and this result was likewise derived from my experiments.

The Energy in a whole System of Material Points.

Let us next advert to the motion of a whole system of material points whose masses we may denote by m, m', m'', &c.

After the lapse of time t, let x, y, z; x', y', z'; x'', y'', z'', &c. be the coordinates of the points m, m', m'', &c., the accelerating forces in direction of the axes for these points respectively X, Y, Z; X', Y', Z'; X'', Y'', Z'', &c., and let the increments of the energies which are yielded by these points to the material resistances be respectively dq, dq', dq'', &c.; then, in consequence of formula (5), we get

$$dq = m \left[\left(X - \frac{d^2 x}{dt^2} \right) dx + \left(Y - \frac{d^2 y}{dt^2} \right) dy + \left(Z - \frac{d^2 z}{dt^2} \right) dz \right],$$

$$dq' = m' \left[\left(X' - \frac{d^2 x'}{dt^2} \right) dx' + \left(Y' - \frac{d^2 y'}{dt^2} \right) dy' + \left(Z' - \frac{d^2 z'}{dt^2} \right) dz' \right],$$

$$dq'' = m'' \left[\left(X'' - \frac{d^2 x''}{dt^2} \right) dx'' + \left(Y'' - \frac{d^2 y''}{dt^2} \right) dy'' + \left(Z'' - \frac{d^2 z''}{dt^2} \right) dz'' \right],$$
&c.

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(3), which had then first to be subtracted from dQ in order to show the increment of the lost energy, or the energy dq appearing in a new shape: partly I have wished thereby to avoid the confounding of material resistances with real forces; for it seems to me that the material resistances are, as it were, "a lifeless thing," to which some part of real force, being the resultant of the three forces X, Y, Z in formula (1) resolved in direction of the path, is imparted during the motion of the mass m. Though it is sure that formula (6) may be regarded as a simple result of formula (1), yet I keep this formula so much the more, that the train of ideas developed above made me at first sensible of the real state of the whole.

If all these last equations are added, and if we put $dq + dq' + dq''$ $+ \ldots = dq_l$, then we get

$$dq_l = \Sigma m \left[\left(X - \frac{d^2x}{dt^2} \right) dx + \left(Y - \frac{d^2y}{dt^2} \right) dy + \left(Z - \frac{d^2z}{dt^2} \right) dz \right], \quad (8)$$

in which dq_l denotes the whole increment of energy which all the material parts together yield to the material resistances, and Σ denotes summation.

If there are no other resistances than those given in the system of material points in question, we have

$$\Sigma m \left[\left(X - \frac{d^2x}{dt^2} \right) dx + \left(Y - \frac{d^2y}{dt^2} \right) dy + \left(Z - \frac{d^2z}{dt^2} \right) dz \right] = 0,$$

which shows that the system does not lose any energy on account of the internal resistances.

The energy imparted to the material resistances by the system during the time t may, conformably to formula (8), be expressed by

$$q_l = \Sigma m \int \left(X dx + Y dy + Z dz \right) - \Sigma \frac{m}{2} \left(\frac{dx^2 + dy^2 + dz^2}{dt^2} \right) + \text{const.} \, (9)$$

If now we compare what has been set forth in equations (1) to (7) above, then we perceive that the whole internal energy which is contained in a system of material points may in all cases be represented by

$$w = \tfrac{1}{2} \Sigma m \left(\frac{dx^2 + dy^2 + dz^2}{dt^2} \right) + C, \quad \cdot \quad \cdot \quad \cdot \quad (10)$$

C being an arbitrary constant.

Hence results *that when the energy of a body manifests itself under the shape of heat, then the contained quantity of heat may always be expressed by the* vis viva *contained in the material particles of the body, as we mean by* vis viva *half the amount of all the masses of the material particles, each multiplied by the square of its own velocity.*

In a note by Ampère, "Sur la Chaleur et sur la Lumière considérés comme résultant de mouvemens vibratoires "*, the author has set forth the idea that, while all rays of light and heat advance in waves through the æther, the propagation of heat in bodies depends upon the vibrations of the atoms and their propagation from particle into particle. Thus, looking upon heat as a motion of atoms, the author compares the quantity of heat contained in bodies with the *vis viva* of the atoms, and thereafter shows that the general equations for the propagation of heat in a body must also hold true for the propagation of the *vis viva*. As I think I have proved in the above that the internal energy of a body must necessarily be equal to the *vis viva* contained in the

* *Annales de Chimie et de Physique*, vol. lviii. p. 432.

particles, it also necessarily results from this that it is by no means disagreeing with nature to apply the stated principle to the propagation of heat in bodies, as, on the contrary, it leads to truths proved by experience.

Now I shall proceed to examine how the internal quantity of energy contained in a fluid must vary when the pressure and density of the fluid vary.

Let dm be the element of the liquid mass m, whose particles, according to the above, must be supposed to be in incessant internal vibration; let the coordinates of the point of mass in question, after the time t, be x, y, z, and let Xdm, Ydm, Zdm be the moving forces on dm in the direction of the three rectangular coordinate axes; further, let the density at this instant for the said point of mass m be ρ, and let p be the pressure on the unit of surface; if moreover the velocities of the element dm in the directions of the three coordinate axes be denoted by

$$u = \frac{dx}{dt}, \quad v = \frac{dy}{dt}, \quad w = \frac{dz}{dt},$$

and if the increments of the velocities during the time dt are equated to
$$u'dt, \quad v'dt, \quad w'dt,$$

then, in conformity to the above, the increment of mechanical energy which the element dm would have received during the time dt if it had been perfectly free will be

$$dm\,(Xdx + Ydy + Zdz).$$

But, as the element dm is not perfectly free, in fact it only receives an increment which may be represented by

$$dm(u'dx + v'dy + w'dz).$$

During the time-element dt this element of mass consequently loses some part of the mechanical energy which is really produced through the accelerating forces. If the energy which dm loses during the time t is represented by $q \cdot dm$, then the energy lost in the time-element dt is equal to $dq \cdot dm$, and thus we get

$$dq \cdot dm = [(X - u')dx + (Y - v')dy + (Z - w')dz]dm. \quad . \quad (11)$$

But this internal energy $dq \cdot dm$, which is imparted to the material resistances by the element dm during the time dt, may be put in a simpler form; for, as is well known, we have

$$\left.\begin{array}{l} dx\,dy\,dz \cdot \dfrac{dp}{dx} = (X - u')\,dm, \\[2mm] dx\,dy\,dz \cdot \dfrac{dp}{dy} = (Y - v')dm, \\[2mm] dx\,dy\,dz \cdot \dfrac{dp}{dz} = (Z - w')dm\,; \end{array}\right\} \quad . \quad . \quad (12)$$

for if the three equations (12) are added after having been multiplied respectively by dx, dy, dz, and we observe that

$$\frac{dp}{dx}\,dx + \frac{dp}{dy}\,dy + \frac{dp}{dz}\,dz = dp,$$

then we see that the formula may be written simply

$$dq \,.\, dm = dx\,dy\,dz \,.\, dp. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

The new increment of energy developed in the unit of mass in the time dt may consequently, for the point in question, be expressed by

$$dq = \frac{dx\,dy\,dz}{dm} \,.\, dp = \frac{1}{\rho} \,.\, dp, \quad . \quad . \quad . \quad . \quad (14)$$

since $dm = \rho \,.\, dx\,dy\,dz$.

By means of formula (14) we are now without difficulty able to determine the amount of internal energy produced in a unit of mass of a liquid body when it is compressed through external force; and as the internal energy produced thereby chiefly appears in the shape of energy of heat, we are able to determine the quantity of heat produced by the compression of fluids.

With regard to this, I shall here call attention to the quantity of heat developed in aëriform bodies suffering compression.

Let us suppose that the gas in question, during the state of equilibrium, has in every place the same density D, and that h and gmh denote the barometric height and the pressure of air answering to this density, g being the force of gravity and m the density of the mercury. Let us further, at any instant during the compression, denote the density and pressure of the gas by ρ and p, then we have

$$\rho = \text{D}(1 + s), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

in which s or the degree of condensation may be either positive or negative.

If the condensation takes place so quickly that heat is neither lost nor received during the motion, and s is only a very small magnitude, then, as is known,

$$p = gmh(1 + \gamma \,.\, s), \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where γ denotes the ratio between the specific heat at constant pressure and that at constant volume. From this formula, whose correctness increases in the same degree as s is decreased, follows,

$$dp = gmh \,.\, \gamma \,.\, ds;$$

and by substituting this value for dp, together with the expression for ρ of formula (15), in the equation (14), we get

$$dq = \frac{gmh}{\text{D}} \,.\, \gamma \frac{ds}{1 + s}.$$

If this equation is integrated, and we observe that s is always supposed to be very small, then we have without any appreciable error

$$q = q_0 + \frac{gmh}{D} \cdot \gamma \cdot s, \quad \cdots \cdots \quad (17)$$

supposing $q = q_0$ for $s = 0$.

If the temperature of the gas in its original state of equilibrium at the density D be denoted by T, and the temperature during the compression at the instant in question be denoted by $(T + \theta)$, then, if the coefficient of expansion of the air is α, we have

$$p = \frac{gmh}{D} \rho \frac{1 + \alpha(T + \theta)}{1 + \alpha T}.$$

When we here substitute the values of ρ and p, according to the formulæ (15) and (16), we get without appreciable error

$$s = \frac{\alpha \theta}{(1 + \alpha T)(\gamma - 1)},$$

which, when substituted in formulæ (17), gives

$$q = q_0 + \frac{gmh \cdot \gamma \cdot \alpha \theta}{D(1 + \alpha T)(\gamma - 1)}.$$

If the density of the gas at $0°$ during the pressure gmh is equated to D_0, then is

$$D(1 + \alpha T) = D_0,$$

and consequently we have

$$q = q_0 + \frac{gmh}{D_0} \cdot \frac{\gamma}{\gamma - 1} \cdot \alpha \theta. \quad \cdots \cdots \quad (18)$$

If the velocity by which q varies in proportion to the temperature is denoted by ω, which represents the specific heat of the fluid of variable volume, then we have

$$\omega = \frac{dq}{d\theta}. \quad \cdots \cdots \cdots \cdots \quad (19)$$

By differentiating the equation (18) with regard to θ, we then get

$$\omega = \frac{gmh}{D_0} \cdot \frac{\gamma}{\gamma - 1} \cdot \alpha; \quad \cdots \cdots \quad (20)$$

and as this expression is not changed, however small s is supposed to be, we perceive that formula (20) must represent the exact expression for the specific heat of variable volume.

When now we denote the specific heat of variable volume for another gas by ω', and the density at $0°$ under the pressure gmh is denoted by D'_0, and the ratio between the specific heat of this

gas at constant pressure (*i. e.* of variable volume) and at constant volume is denoted by γ, then, as the coefficient of expansion α is the same for all gases, we find

$$\omega' = \frac{gmh}{D'_0} \cdot \frac{\gamma'}{\gamma'-1} \cdot \alpha\,;$$

and by taking the ratio between the quantities of specific heat for these gases, we get

$$\frac{\omega}{\omega'} = \frac{D'_0}{D_0} \cdot \frac{\gamma}{\gamma-1} \cdot \frac{\gamma'-1}{\gamma'}, \quad . \quad . \quad . \quad (21)$$

which exactly is Dulong's formula according to which the specific heat of the gases is calculated*.

I shall briefly make use of these formulæ to determine the development of heat which takes place during the propagation of sound in an aëriform body.

If the velocity of the sound be denoted by a, then, according to Poisson, we have

$$a = \sqrt{\frac{gmh}{D}} \cdot \gamma$$

when we keep the same designations which are used above; and if the aëriform body is imagined to be unlimited in all directions round a fixed point (the original point of the coordinates at which the undulation commences), and if, after the lapse of time t, r is used to denote the radius vector of the point whose coordinates are x, y, z, then the degree of condensation s in this point and at this instant is determined by the equation

$$s = \frac{1}{ar} \left[F(r-at) - f(r+at) \right],$$

F and f representing two arbitrary functions; if this expression for s is substituted in the equation (17), we get the quantity of heat developed,

$$q = \frac{a}{r} \left[F(r-at) - f(r+at) \right]. \quad . \quad . \quad . \quad (22)$$

Next let us examine the quantity of heat developed through compression of liquid bodies.

Here it will be convenient to start from Œrsted's experiments upon the compression of fluids. Agreeably to the said experiments, it may be taken for granted that when a fluid for one atmosphere of pressure is compressed by a fraction of volume equal to β, then this fluid is compressed 2β, 3β, 4β, &c. by the pressure of 2, 3, 4, &c. atmospheres.

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* See *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, vol. x. p. 188.

From Œrsted's later experiments upon development of heat through compression of water, we may infer approximately that the development of heat is proportional to the pressure, so that 2, 3, 4, &c. times as much heat is developed by 2, 3, 4, &c. atmospheres of pressure as by 1 atmosphere of pressure.

If we then imagine a unit of mass of a certain liquid, and we suppose its density $= D'$ and its volume $= V'$ at the temperature T', and if we put the pressure on unit of surface $= gmh$, and if we further suppose that the pressure is changed and becomes $= p'$, then the temperature rises to $(T' + \theta')$, the density becomes ρ', and the volume becomes V'. We thus have

$$\rho' = D'(1 + s'), \quad \ldots \quad \ldots \quad (23)$$

s' denoting the degree of condensation. But s' always being very small, we have with sufficient approximation

$$V'_i = V'(1 - s'). \quad \ldots \quad \ldots \quad (24)$$

If, further, the coefficient of compression for one atmosphere at the temperature T' is denoted by β, then, conformably to Œrsted's experiments, we have

$$\left.\begin{array}{l} V'_i = \left[1 - \left(\dfrac{p'}{gmh} - 1\right)\beta\right] V', \\[2mm] \theta' = \left(\dfrac{p'}{gmh} - 1\right) \cdot \epsilon', \end{array}\right\} \quad \ldots \quad (25)$$

since the pressure of air gmh is supposed to be equal to one atmosphere, and the development of temperature for one atmosphere of pressure is denoted by ϵ'.

If both equations (25) are resolved with regard to p', and if formula (24) is taken into account, then we have

$$\left.\begin{array}{l} p' = gmh\left(1 + \dfrac{1}{\beta} \cdot s'\right), \\[2mm] p' = gmh\left(1 + \dfrac{\theta'}{\epsilon'}\right), \end{array}\right\} \quad \ldots \quad \ldots \quad (26)$$

whence follows

$$s' = \beta \frac{\theta'}{\epsilon'},$$

which, substituted in the equation (23), gives

$$\rho' = D'\left(1 + \beta \frac{\theta'}{\epsilon'}\right).$$

When the second equation (26) is differentiated, then we get

$$dp' = gmh \cdot \frac{d\theta'}{\epsilon'}.$$

The increment of heat which the body receives while the pressure passes from p' to $p'+dp'$ will be, according to formula (14),

$$dq' = \frac{gmh}{D' \cdot \beta} \cdot d \cdot \log\left(1 + \beta \frac{\theta'}{\epsilon'}\right),$$

the integral of which, with sufficient approximation, may be written

$$q' = q'_0 + \frac{gmh}{D'} \cdot \frac{\theta'}{\epsilon'}, \quad . \quad . \quad .. \quad . \quad . \quad . \quad (27)$$

when we suppose $q' = q'_0$ for $\theta' = 0$.

Hence is derived the specific heat of the fluid,

$$\omega_l = \frac{gmh}{D'} \cdot \frac{1}{\epsilon'} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

By comparing the specific heat of a gas, formula (20), with the specific heat of a fluid, formula (28), we find

$$\frac{\omega}{\omega_l} = \frac{D'}{D_0} \cdot \frac{\gamma}{\gamma - 1} \cdot \alpha\epsilon';$$

or if the density of the fluid at $0°$ is denoted by D'_0, then is

$$D'_0 = U \cdot D',$$

U being the known function of the temperature T' which represents the law of the expansion of the fluid by heat under constant pressure. On account of this the above equation may be written

$$\frac{\omega}{\omega_l} = \frac{D'_0}{D_0} \cdot \frac{\gamma}{\gamma - 1} \cdot \frac{\alpha\epsilon'}{U} \cdot \quad . \quad . \quad . \quad . \quad . \quad (29)$$

Suppose now, as a particular case, that the gas in question is atmospheric air and that the fluid is distilled water, both at the temperature $0°$, then is

$$U = 1, \quad \frac{\omega}{\omega_l} = 0{\cdot}2669, \quad \frac{D_0}{D'_0} = 0{\cdot}001299, \text{ and } \alpha = 0{\cdot}00366;$$

further, in consequence of the best observations upon the velocity of sound at $15°{\cdot}9$ C., $\gamma = 1{\cdot}407$. When these values are substituted and the equation is resolved with regard to ϵ', then we find

$$\epsilon' = \frac{1}{36{\cdot}57} \text{ degree Celsius,}$$

which development of heat exactly agrees with that derived from some experiments which Œrsted made a few years ago upon the compressibility of water at different temperatures.

If the specific quantities of heat for two fluids at the tempera-

ture T' are compared, we have, agreeably to formula (28),

$$\frac{\omega_l}{\omega_{ll}} = \frac{D''}{D'} \cdot \frac{\epsilon''}{\epsilon'}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

ω_l, D', ϵ', as before, denoting the specific heat, the density, and the heat developed through one atmosphere for one of the bodies, and ω_{ll}, D'', ϵ'' denoting the magnitudes for the other fluid analogous to ω_l, D', ϵ'.

If ω_{ll}, D'', and ϵ'' are known for one of the fluids, as, for example, is the case with distilled water, in which

$$\omega_{ll} = 1, \quad D'' = 1, \text{ and } \epsilon'' = \frac{1}{36 \cdot 57},$$

and if the amounts ω_l and D' are known for the other fluid, then formula (30) serves to determine the degree of heat ϵ' which will be developed in this fluid by one atmosphere of pressure; for we find

$$\epsilon' = \frac{1}{36 \cdot 57 \cdot D' \cdot \omega_l}. \quad . \quad . \quad . \quad . \quad (31)$$

For the following liquid bodies I have in this manner determined the degree of heat ϵ' which would be developed in case these fluids were subjected to one atmosphere of compression :—

Name of the fluid.	The density.	The specific heat.	The calculated degree of heat ϵ'.
Distilled water . .	1·000	1·000	$\left(\dfrac{1}{36 \cdot 57}\right)^{\circ}$ Celsius.
Sulphuric acid . .	1·848	0·335	$\left(\dfrac{1}{22 \cdot 64}\right)$,,
Alcohol	0·793	0·700	$\left(\dfrac{1}{20 \cdot 30}\right)$,,
Olive-oil	0·915	0·504	$\left(\dfrac{1}{16 \cdot 86}\right)$,,
Mercury	13·598	0·0333	$\left(\dfrac{1}{16 \cdot 56}\right)$,,
Sulphide of carbon.	1·272	0·329	$\left(\dfrac{1}{15 \cdot 30}\right)$,,
Bromine	2·966	0·135	$\left(\dfrac{1}{14 \cdot 64}\right)$,,
Sulphuric ether . .	0·715	0·550	$\left(\dfrac{1}{14 \cdot 38}\right)$,,
Oil of turpentine .	0·872	0·426	$\left(\dfrac{1}{13 \cdot 58}\right)$,,

The degree of heat developed in a fluid by one atmosphere of pressure will in general be a function of the temperature of the fluid. If, for example, we take distilled water and suppose its specific heat to be unvaried at all temperatures, then, according to formula (28), we get, at the temperature T',

$$\omega_1 = gmh \frac{1}{D' \cdot \epsilon'};$$

and at the temperature $0°$,

$$\omega_1 = gmh \frac{1}{D'_0 \cdot \epsilon'_0},$$

D'_0 and ϵ'_0 denoting the values of D' and ϵ' for $T' = 0°$.

Whence, consequently, follows

$$\epsilon' = \epsilon'_0 \frac{D'_0}{D'},$$

which shows that for water the degree of heat developed varies so little that, on the whole, it may be considered constant.

Conformably to formula (20), we may now easily determine the amount of mechanical energy equivalent to the unit for quantities of heat, as one unit of heat raises the temperature of 1 pound of water 1 degree Celsius ; for this formula may be written

$$\omega = gmh (\alpha V_0) \frac{\gamma}{\gamma - 1},$$

as we notice that, when the volume for the said unit of mass of air at $0°$ under the pressure gmh is denoted by V_0, then

$$D_0 V_0 = 1.$$

But now

$$gmh = \frac{0 \cdot 76^m \cdot 13 \cdot 598 \cdot 62 \text{ pounds}}{1728};$$

and if the mass of one pound of air is taken as unit, then is

$$\alpha V_0 = \frac{0 \cdot 00366 \cdot 1728}{0 \cdot 001299 \cdot 62};$$

also

$$\gamma = 1 \cdot 407 \text{ and } 0 \cdot 76 \text{ metre} = 2 \cdot 421 \text{ feet},$$

whence follows

$$\omega = 321 \cdot 42 \text{ pounds} ; \quad \cdots \quad (32)$$

which shows *that when mechanical energy, expressed by* 1 *pound raised to the height of* 321·42 *feet, is imparted to one pound of air, then the internal energy of the air will be increased in such a manner that its temperature must rise one degree Celsius.* If the specific heat of the water is denoted by ω_1, then, in conformity to De la

Roche and Berard,

$$\omega_I = \frac{\omega}{0.2669},$$

from which results that *the mechanical energy equivalent to the energy of heat in a unit quantity of heat* is

$$\omega_I = 1204\cdot3 \text{ pounds.} \quad . \quad . \quad . \quad . \quad . \quad (33)$$

This expression for the specific heat of water shows that when the quantity of heat which is able to heat 1 pound of water 1 degree Celsius (the so-called unit of heat) is used in the most profitable manner for producing mechanical energy, then 1204·3 units of work may be produced from it, as a unit of work is equated to 1 foot-pound; and conversely, when an energy $= 1204\cdot3$ units of work is imparted to the material particles of a body, then the internal energy among the particles, when appearing as energy of heat, will be increased by exactly one unit of heat.

When this result is compared with what I formerly derived from my experiments upon the heat produced by friction of solid bodies, by which I found an average of 1 unit of heat equal to 1185·4 units of work*, then we perceive that this average differs a little from that represented in (33)—however, not more than might be expected from the few experiments which I have hitherto had opportunity to undertake†.

In the preceding we have examined the quantity of energy produced in a fluid undergoing compression ; let us now proceed to determine the general expression for the magnitude of the energy contained in a fluid at a given temperature, pressure, and density.

If the material points of which the fluid consist are (as above) denoted by m, m', m'', &c., their coordinates by x, y, z ; x', y', z' ; x'', y'', z'', &c., and the accelerating forces, by which these points are moved, by X, Y, Z ; X', Y', Z' ; X'', Y'', Z'', &c., and if we suppose that the fluid by degrees yields some part of its energy, under the form of mechanical energy, for the production of a certain work, then, in conformity to the formulæ (9) and (10), the whole quantity of energy which the fluid has lost, after the lapse of time t, may be expressed by

$$q = \Sigma m \int (Xdx + Ydy + Zdz) - w + C, \quad . \quad . \quad (34)$$

as $\Sigma m \int (Xdx + Ydy + Zdz)$ denotes the sum of all terms analogous to $m \int (Xdx + Ydy + Zdz)$ answering to all the material

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* See *Vidensb. Selsk. Skr.* 5 Række, naturv. og math. p. 146.

† Experiments upon this subject have lately been made by Mr. J. P. Joule, Pogg. *Ann.* vol. lxxiii. p. 479.

points m, m', m'', &c., and w represents the energy which the fluid actually contains.

Provided the fluid is a gas, we may without any appreciable error leave out of the formula the terms which depend on the mutual attraction of the particles of air, and then the formula may be written

$$q = C - w. \qquad \qquad (35)$$

When the mass of air by which the work is performed is supposed to be $= \mu$, then, conformably to formula (14), we have, by differentiation of the equation (35),

$$\mu \frac{dp}{\rho} = -dw.$$

But, in accordance with Mariotte and Gay-Lussac's law, ρ is a given function of p and θ, determined by the equation

$$p = k\rho(1 + \alpha\theta), \qquad \qquad (36)$$

θ being the temperature Celsius, and α the coefficient of expansion for the gas, and k the ratio between the pressure of air gmh and the density D_0 at $0°$. Thus we find

$$\mu k(1 + \alpha\theta)\frac{dp}{p} + dw = 0,$$

the whole integral of which is

$$w = f(\theta) - \mu k(1 + \alpha\theta)\log\frac{p}{p_0}, \qquad \cdot \quad \cdot \quad \cdot \quad (37)$$

$f(\theta)$ denoting an arbitrary function of θ, p_0 any constant pressure, and log represents the natural logarithm.

Formula (37) is exactly the same as Holtzmann has derived for vapour (of water)* by a procedure similar to that which was first stated by Clapeyron†.

As is well known, the specific heat at constant pressure is derived from this formula,

$$\omega = \frac{1}{\mu}f'(\theta) - k\alpha\log\frac{p}{p_0}, \qquad \cdot \quad \cdot \quad \cdot \quad (38)$$

and the specific heat at constant volume will be

$$\omega_2 = \frac{1}{\mu}f'(\theta) \div k\alpha\log\frac{p}{p_0} \div k\alpha, \qquad \cdot \quad \cdot \quad (39)$$

in which $f'(\theta)$ denotes the differential coefficient of $f(\theta)$ with regard to θ. From formulæ (38) and (39) we may also easily derive formula (20).

If steam is at maximum of density, and the quantity of heat w contained in the same mass of steam is supposed to be constant, then, conformably to formula (37), the pressure is a function

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* Pogg. *Ann. d. Physik*, Ergänzungsband ii. p. 183.
† Ibid. vol. lix. p. 446.

of the temperature only, viz.

$$\log\frac{p}{p^0} = \frac{f(\theta) - w}{\mu k(1 + \alpha\theta)}.$$

If we take the total differential of the right member of equation (37) in a case where w is constant, then it must be $= 0$; we consequently get

$$\left(f'(\theta) - \mu k\alpha \log\frac{p}{p_0}\right)d\theta - \mu k(1 + \alpha\theta)\frac{dp}{p} = 0;$$

but according to the formulæ (20) and (38)

$$f'(\theta) - \mu k\alpha \log\frac{p}{p_0} = \mu\frac{gmh}{D_0}\frac{\gamma}{\gamma - 1}\cdot\alpha,$$

which, substituted above, gives

$$\alpha\cdot\frac{\gamma}{\gamma - 1}\,d\theta = (1 + \alpha\theta)\frac{dp}{p},$$

whence follows

$$\frac{dp}{p} = \frac{\alpha d\theta}{\dfrac{\gamma - 1}{\gamma}(1 + \alpha\theta)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (40)$$

This differential equation for the elastic force of steam in proportion to the temperature, when the steam is at maximum density, is exactly what Baron Wrede has earlier found; but as this formula has been criticised in Dove's *Repertorium der Physik*, vol. vii. p. 231, as not being exact, a direct proof of its correctness, under the supposition of w being constant, will perhaps not be unnecessary.

It is known that Poisson, by means of formula (36), has proved that when the quantity of heat contained in an aëriform body is denoted by w, then w must be such a function of p and ρ that it satisfies the differential equation

$$\gamma\cdot p\frac{dw}{dp} + \rho\frac{dw}{d\rho} = 0,$$

γ, p, and ρ having the same signification as above.

But we know that this equation is integrated by putting

$$\rho dp - \gamma p d\rho = 0 \text{ and } dw = 0;$$

for if the integrals of these two equations are respectively denoted by

$$M = a \text{ and } w = b,$$

when they are supposed to be resolved with regard to the arbitrary constants a and b, then we know that

$$M = F(w),$$

in which $\mathrm{F}(w)$ denotes an arbitrary function of w, represents the complete integral of the preceding partial differential equation.

But for steam at maximum of density we suppose w, and accordingly also $\mathrm{F}(w)$, constant; then $\mathrm{M} = $ constant; whence follows that for steam at maximum of density $d\mathrm{M} = 0$, or

$$\rho dp - \gamma p\, d\rho = 0\;;$$

consequently

$$\frac{dp}{p} = \gamma \cdot \frac{d\rho}{\rho}.$$

If this equation is compared with the logarithmic differential of formula (36), we find

$$(\gamma - 1)\frac{dp}{p} = \gamma \frac{\alpha d\theta}{1 + \alpha\theta},$$

whence follows

$$\frac{dp}{p} \frac{\alpha d\theta}{\dfrac{\gamma - 1}{\gamma}(1 + \alpha\theta)}.$$

Consequently this appears to me to be at the same time an incontrovertible proof of the validity of Baron Wrede's formula, and of the correctness of the principle laid down here.

[As many of Joule's early papers obtained publicity in the Philosophical Magazine, and as those of Mayer have also appeared in its pages, I have thought that the above translation (which has recently been made for me) of one of Colding's memoirs (of date 1850) on the same subject might with propriety be inserted in the same Journal. There are other papers by Colding, of at least equal importance, which may perhaps also appear in English.—P. G. Tait.]

II. *Arrangement for the Discharge of long Overland Telegraph Lines.* By Louis Schwendler, *Esq.*[*]

WHEN organizing more regular and instantaneous telegraphic communication between the Presidency towns of India, and especially between Calcutta and Kurrachee, it was observed that discharges occurred sufficiently strong to affect the relay of the sending station, and giving rise to the so-called "return beats." These discharges[†], through the relay of the

[*] From the Journal of the Asiatic Society of Bengal, vol. xl. part 2 (1871). Communicated by the Author.

[†] It is well known that an overland telegraph-line acts as a **Leyden jar** in the same manner as a submarine cable, having, however, only a much smaller capacity, on account of the insulating layer (the air between the telegraph-wire and surrounding conductors) being very thick. But though the capacity may be small in comparison with that of any cable, it is evident that a long, well-insulated overland line may show nevertheless

sending station, are inconvenient for many reasons; the most important of which is, that they are frequently stronger than the singnalling current of a far distant station, and consequently throw the relay out of its adjustment, and so make it unfit to receive a calling signal from such a station. It was therefore necessary to devise some simple means by which these discharge-currents could be safely eliminated from the relay of the sending station*; and it was found that for terminal stations a peculiarly constructed key answered the purpose best. This key, after each signal sent, by a proper application of well-tempered springs, makes a momentary contact direct with earth, by which the discharge of the line is effected before the final contact with earth through the relay is made; and such keys were supplied to the terminal stations of the Indian main lines, where they have worked well. But to eliminate the discharge-currents from the relays of terminal stations is of far less importance than to do so from the relays at translation stations; for it is clear that the discharges in translation stations may not only be inconvenient, but may momentarily interrupt the line, so that the real signal cannot pass on; and even if they do not cause interruption during the whole of a signal, they will, at all events, produce points instead of bars at the receiving station, thereby causing considerable delay and confusion.

It is true that in principle the arrangement in use at terminal stations might also be applied at translation stations, where the

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very decided charges and discharges. Fortunately the charges of the Indian main lines (so long in comparison with the direct-worked lines in Europe) still occupy such a short time as not to influence in the least our maximum working speed attainable with the present signalling system (25 to 30 words a minute); *i. e.* a signal sent from Calcutta to Agra arrives there practically at the very moment it is sent. The discharges, however, affect most seriously our instruments; and it is therefore only this effect that is treated of in the present paper.

* The method of a station permanently cutting out its own relay while sending has never been adopted in this country, and I believe also never will be; for however perfect lines and instruments, and accomplished employees may be, or may become, it is always highly desirable that a receiving station should be able to call in the sending station at any moment during the transmission of a message.

In India we invariably use positive currents (or copper to line) for signalling, because they reduce the leakage. By using positive currents for signalling in one direction and negative currents in the other, and having polarized receiving instruments, the effect of discharges would be, of course, so far eliminated that the receiving instruments would not actually be worked by them, the discharges going in the wrong direction through the polarized relays. But this is a bad plan. The continued passage of strong discharges through a polarized relay make it, on account of remanent magnetism, unsensitive, and consequently a continual and most tedious adjustment of the receiving relay would be necessitated; this, again, would produce great irregularity in the working of the lines.

armatures of the sounders, or any other receiving instruments, act as keys; but there are many mechanical difficulties in the way, especially the very small play of these armatures, which would make such a method unsafe. It was therefore decided to use for translation stations another discharging arrangement, which I will now describe. This arrangement consists of a Siemens's polarized relay with comparatively small resistance, and of a small bobbin of wire acting as a shunt to the coils of the relay, which latter may appropriately be called the " discharging relay." The parallel circuit of discharging relay and bobbin of wire is interposed between the line to be discharged after each signal and the sending battery.

The contact-screw of the discharging relay is connected with one end of the receiving relay, while the axis of the tongue of the discharging relay is in connexion with the other end of the receiving relay, *i. e.* the earth. Such an arrangement may be, of course, applied equally well for terminal stations in place of a discharging key; and as the telegraph circuit for two terminal stations is of a simpler nature than the translation circuit, it will be clearer to explain the action of this discharging arrangement for two terminal stations working direct with each other, as, for instance, Calcutta and Agra.

The following diagram (fig. 1) will give all the necessary connexions.

Fig. 1.
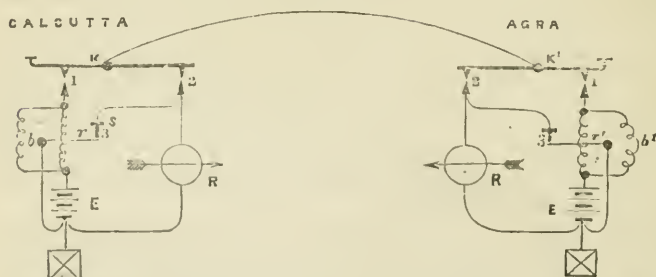
R and R' are the receiving relays, the tongues of which, when a current is sent, close the circuit of a local battery containing the receiving instrument in the usual manner.

K and K' are two common telegraph keys, *r* and *r'* the two discharging relays, *b* and *b'* the two bobbins of wire acting as shunts to *r* and *r'* respectively.

Suppose Calcutta sends a signal to Agra by pressing the key K on its front contact 1; then a part of the Calcutta signalling current passes through *r*, and, if strong enough, attracts the relay tongue, pressing it against the contact-screw S; and as long as contact 1 lasts, contact 3 will exist. But as soon as the

signal is completed (*i. e.* when the key leaves contact 1 and makes contact 2) all the discharge of the long line would pass through the receiving relay R if contact 3 ceased just before contact 2 were reestablished. This, however, is not the case, because by the application of the shunt *b*, in virtue of which an extra current can form itself through the coils of the discharging relay, contact 3 is sufficiently prolonged to exist for a moment simultaneously with contact 2; consequently the whole discharge, or at least the greatest part of it, has time to pass through contact 3 direct to earth, instead of going through the receiving relay R. The same process will, of course, repeat itself at each signal sent, and will also be the case when Agra is sending instead of Calcutta.

Such an arrangement answers the purpose perfectly at Agra on the great and important main line between Calcutta and Kurrachee, where it has been in use (in translation) for some time.

It may be mentioned here that it does not at all interfere with the maximum working speed attainable with our present system of signalling, namely 25–30 words a minute.

The very great prolonging-power of such a shunt not having been known at first, it was thought necessary to assist the prolonging effect by a fine spring of very small play fixed to the tongue of the *discharging relay*. But such a spring is not wanted; and it is much better to dispense with it, because, however small the play of this contact spring may be made, it will always in some measure lessen the sensitive adjustment of the discharging relay.

Fig. 2.
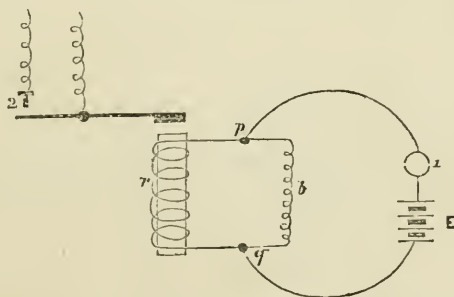
As it was evident that the prolonging effect of the shunt must greatly depend upon its resistance (supposing the resistance of the discharging relay and also all other circumstances were given), the following investigation was made in order to ascertain its amount.

Fig. 2 represents the simple circuit as obtained from fig. 1.

Two bobbins of wire, r and b, are connected parallel to the two poles of a battery, E, the circuit of which may be closed and opened at will by a stopper, 1. Only one of the bobbins (r for instance) contains iron, which becomes a magnet as soon as the battery circuit is closed. When the circuit is opened the magnetism in r ceases, causing an extra current in $(r+b)$ which acts in the same direction as the original battery current, and consequently causes the loss of magnetism in r to go on much slower than it would without such a shunt b; and therefore, if the magnetism in r were made use of for closing a contact, 2, this contact would be somewhat prolonged by such a shunt. Consequently the question to be solved is, what must be the resistance of this shunt, supposing r and every thing else were given, in order to make the remanent magnetism a maximum, *i. e.* the prolonging effect of the shunt as regards contact 2 greatest.

That for a given r a certain b does exist for which the extra current, or its equivalent the remanent magnetism, is greatest follows simply enough. Suppose, for instance, the resistance of the shunt b were infinite, which is the same as having no shunt at all, then no extra current would exist, though its cause (*i. e.* the magnetism produced in r by closing the battery circuit) would be greatest. On the other hand, if the resistance of the shunt b were taken as infinitely small, then (though there would be the best possible channel for an extra current) no such current could be established, because no original current would pass through r, and therefore no magnetism in r could have been developed. Knowing, therefore, that for $b=\infty$ and for $b=0$ the extra current is $=0$, it follows that there must be one or more values of b between these limits for which the extra current is a maximum.

However, the function by which this extra current (or, better, the remanent magnetism in r) is expressed is of such a nature that it has only *one* maximum; and this can easily be calculated, since all the laws determining it are perfectly known.

In fig. 2 we will designate by r the resistance of the bobbin producing the magnetism (which in the discharging arrangement represents the resistance of the coils of the discharging relay); and for brevity we may suppose that the whole resistance between the points p and q through r is used for producing magnetism.

Suppose also :—

n the number of convolutions in r;

x the resistance of the bobbin acting as a shunt to r, and extending between p and q;

E the electromotive force producing the original current;

l the resistance between the points p and q through the battery E, including the resistance of the battery;

thus the current C which passes through r when 1 is closed is

$$C = E \frac{x}{l(r+x)+rx},$$

and consequently the magnetism m developed in r by C is

$$m = Cn = E \frac{nx}{l(r+x)+rx};$$

and supposing the conductivity of the wire filling the given space of bobbin r constant for any diameter whatever, and neglecting the thickness of the necessary insulating covering of the wire in comparison with its diameter, we may substitute for n the value $n = \text{constant} \sqrt{r}$.

Thus we have

$$m = E \text{ const.} \frac{x\sqrt{r}}{l(r+x)+rx}.$$

The ceasing of this magnetism after the battery circuit has been instantaneously opened at 1 must be considered the cause for producing an extra current in the closed circuit $(r+x)$, which extra current in its turn reproduces magnetism in the iron bar in the coil r. This whole process, of course, occupies time, however short it may be, and goes on steadily. But it will lead apparently to the same result for our purpose if we suppose that the cessation of the original magnetism produces instantaneously the whole extra current, and that the extra current (or, better, an average value of it, since it is variable as regards time) is used for producing fresh magnetism in the iron bar of the coil r. Under these circumstances it is reasonable to take a proportional quantity of the original magnetism as the new electromotive force for producing the extra current C' in the circuit with the resistance $r+x$.

Therefore we have

$$C' = E \text{ const.} \frac{x\sqrt{r}}{\{l(r+x)+rx\}(r+x)};$$

and this expression, multiplied by the number of convolutions n, gives us the remanent magnetism m', or, as $n = \text{const.} \sqrt{r}$,

$$m' = E \text{ const.} \frac{xr}{\{l(r+x)+rx\}(r+x)}. \quad \cdot \quad \cdot \quad \cdot \quad \text{(I.)}$$

Now it is evident that the prolonging effect (*i. e.* the time during which the bar of iron keeps perceptibly magnetized after the instantaneous opening of the battery circuit) must increase with m'; and consequently by making m' a maximum the prolonging effect of the arrangement must also be greatest. Taking, therefore, in the above expression for m', x only as variable, we

get in the usual way

$$*x = r\sqrt{\frac{l}{l+r}}, \quad \ldots \quad \ldots \quad \text{(II.)}$$

corresponding to the maximum of m.

In the application to a long overland line, l represents the line resistance, including the resistance of the sending battery and distant receiving relay, while r is the resistance of the coils of the discharging relay.

In order to weaken as little as possible the signalling current by the introduction of such a discharging relay, we take naturally r (its resistance) only so great that a given electromotive force (as is generally used for signalling through the line) will work it with safety through the given line resistance; and if the discharging relay is of a good construction, this r can always be neglected in comparison with l.

Therefore we have from formula (II.),

$$x = r.$$

Or, to make the prolonging effect of the shunt a maximum, its

* We have

$$\frac{dm'}{dx} = \frac{lr^2 - x^2(l+r)}{N^2},$$

where

$$N = \{l(r+x) + rx\}\{r+x\};$$

$$\therefore \frac{d^2m'}{dx^2} = -\frac{2x(l+r)}{N^2} - \frac{2}{N} \cdot \frac{dm}{dx} \cdot \frac{dN}{dx};$$

$\therefore$ when $\dfrac{dm'}{dx} = 0,$

$$x = r\sqrt{\frac{l}{l+r}},$$

and

$$\frac{d^2m'}{dx^2} = -\frac{2x(l+r)}{N^2},$$

which is always negative for a positive value of x.

The function m' (formula I.) is to be considered as representing the remanent magnetism in the closed circuit $(r+x)$, no matter by which of the two coils the magnetism is produced; thus m' must necessarily be symmetrical as regards r and x. But having selected one of the two coils by which m' (the remanent magnetism) *is* to be produced, it is at once fixed which of the two coils must be taken as variable in order to find the maximum of m'. If, for instance, r is taken as the coil developing m', while x acts as shunt only, neither producing extra current nor magnetism, then the shunt x must be taken as variable, and not r, since otherwise factor

$$\frac{r}{\{l(r+x) + rx\}\{r+x\}}$$

would have to be differentiated, giving that value of r which represents a maximum of m' developed by x—just the case to be avoided as much as possible.

resistance must be equal to the resistance of the coils of the discharging relay. This law will hold good for any long overland telegraph line ; and it is only for a long line that such a discharging arrangement is required.

As regards the absolute value of r, it was found that 200 S. units, using a Siemens's polarized relay, were quite large enough. Such a relay works safely with 30 Minotti's cells through 10,000 S. units.

The shunt itself, even without having iron in it, produces an extra current which is in the same direction in the shunt as the primary current, and consequently opposes the extra current produced by the coil r in the closed circuit $(r + x)$.

In order to have, therefore, the action of the coil r not too much lessened by the extra current produced by the shunt, it is necessary to make the latter of the thinnest possible German-silver wire, and wind it on a large bobbin with the convolutions as far distant from one another as possible. Another method would be to wind the bobbin bifilarly.

In conclusion I may mention that the longest main line in India is the one between Calcutta and Kurrachee, 1700 miles in length, which has been worked direct now for more than two years,—Agra (which is about at the middle) only in translation. During the dry season, when the lines up country often have an insulation of more than 200 millions S. units per mile, it is possible to work this enormous distance altogether direct without Agra in translation ; but practically nothing would be gained by this, since then, on account of the great length, the charge becomes so large as to reduce the speed to less than fifteen words a minute, while by having Agra in translation the speed, if only the signalling system would allow of it, would reach to upwards of sixty words a minute.

Note.—Mr. A. Cappel, in his report on the Central London Office of the Electric and International Telegraph Company, states that a shunt in connexion with an electromagnet for discharging one of the cables was made use of as early as 1867, and was announced to him by Mr. Culley as an invention of one of the telegraph clerks. This appears to be the first application of the extra current for this purpose ; but I am not aware whether this simple principle has since been used for overland telegraph lines.

Mr. Cappel says :—" The duration of the zinc current (necessary to neutralize, after each signal sent, the positive discharge of the cable) can be regulated by varying the resistance of the shunt ; but no definite law or conclusion has yet been arrived at on the subject."

III. *On the Application of a new Integration of Differential Equations of the Second Order to some unsolved Problems in the Calculus of Variations.* By the Rev. Professor CHALLIS, *M.A.,* *F.R.S., F.R.A.S.**

BEFORE stating the principle of the proposed new integration, and applying it in an example, it will be proper to direct attention to the following two Lemmas :—

I. The formula $\int y dx$ may express *the area of any curve* between assigned limits of the abscissæ, whatever may be the origin and direction of the rectangular coordinates, and whether y has one or more values for a given value of x, provided always the integration be taken along *consecutive* points of the curve.

II. Under precisely the same conditions $\int \left(1 + \frac{dy^2}{dx^2}\right)^{\frac{1}{2}} dx$ may express *the length of any curve* between assigned limits of the abscissæ, the radical being always taken with a *positive sign.*

To exemplify the first Lemma, suppose the curve to be a circle situated within the quarter for which the signs of x and y are both positive. Then if we begin the integration with the less of the two ordinates corresponding to a given abscissa, and proceed along consecutive points of the curve in the direction of the movement of the hands of a watch till we arrive at the extremity of the other ordinate to the same abscissa, the integration between these limits will be the segment of the circle cut off by this ordinate. For the integration from the given abscissa to the minimum abscissa is *negative,* because through that interval dx is negative ; and the integration from the minimum abscissa to the given abscissa is *positive,* because in the return direction dx is positive. Also the positive area is made up of the negative area and the above-mentioned segment of the circle, so that the total integration gives the area of the segment.

The same principle applies, however complicated the curve may be ; only to effect the required integrations it is necessary to obtain all the real values of y corresponding to any value of x, either exactly or by approximation, as explicit functions of x.

If it be required to calculate the area enclosed by a portion of the curve having its extremities at given points and the chord joining the points, the same rule applies, if, after integrating from one point to the other, the integration be continued *along the chord* to the first point.

With respect to the second Lemma, it is only necessary to remark that when the integration is taken along consecutive

points, the radical and dx change sign together, so that $\left(1 + \dfrac{dy^2}{dx^2}\right)^{\frac{1}{2}} dx$ has always the same sign, which we may assume to be positive.

Exactly the same considerations apply, *mutatis mutandis,* if the curve be referred to polar coordinates ; and in the case of a closed curve the pole may be at any point either within or without it.

The foregoing Lemmas, which ought to be introduced into the Elementary Treatises on the Integral Calculus, I supposed to be new, until it was pointed out to me by Mr. Todhunter on my submitting to him Lemma I., that in his 'History of the Calculus of Variations' (art. 139) an application of the method of integrating along consecutive points of a curve is recorded as having been made by M. Delaunay in a memoir published in 1843. It is evident from this account that M. Delaunay produces the method as a recent discovery made by himself; but the principle of it is not stated by him as generally as it might be, no mention being made of integrating along a chord, or with respect to *intersecting* portions of different curves.

My purpose also requires a few preliminary remarks to be made respecting the principles of the Calculus of Variations. The elementary treatises on this branch of analytics give a method of obtaining a differential equation on the integration of which the solution of the proposed problem depends. But they do not usually take into account that, as in all problems involving two variables x and y it is abstractedly possible to treat the variations δx and δy as independent, and to obtain *two* differential equations, one of these is just as much as the other entitled to be applied in the solution of the problem. Similarly, if there are three variables x, y, z, three equations, all applicable to the problem, are obtainable. (This method of treating the Calculus of Variations is given in Francœur's *Mathématiques Pures,* tom. ii. 2nd ed. art. 885.) The possibility of obtaining more than one differential equation is a significant analytical circumstance which cannot be overlooked without restricting the applications of the calculus. When there are only two variables x and y (as is supposed to be the case in all the subsequent reasoning), it may be shown in the usual manner that

$$(\delta y - p\,\delta x)\left\{ \mathrm{N} - \left(\frac{d\mathrm{P}}{dx}\right) - \left(\frac{d^2\mathrm{Q}}{dx^2}\right) + \&c. \right\} = 0,$$

the differential coefficients being put in brackets to indicate that they are complete (see arts. 8 and 9 of the "Calculus of Variations" in Airy's 'Mathematical Tracts'). Hence, if the problem involves no relation between δy and δx, we have, putting

A for the quantity in brackets, with equal reason

$$\Lambda = 0, \text{ and } \Lambda p = 0.$$

If it might be assumed that these are identical equations, it would suffice to integrate one of them. But, in fact, although in some instances both are immediately integrable and give identical relations between x and y, in others only one, or neither of the two, is immediately integrable; and it cannot, therefore, be antecedently affirmed that they are identical equations. Hence it is necessary to take account of results deducible from them either separately or conjointly. I propose, first, to adduce an instance in which the integrals of the two equations are identical.

Problem 1. Required to connect two fixed points by a curve of given length so that the area bounded by the curve, the ordinates of the fixed points, and the axis of abscissæ shall be a maximum.

According to Lemma I. the area thus defined is equal to $\int y \, dx$ taken from the abscissa of one extremity of the curve to the abscissa of the other; and by Lemma II. the length of the curve is $\int \sqrt{1 + p^2} \, dx$ between the same limits, p being put for $\dfrac{dy}{dx}$. Hence by the Calculus of Variations, a being an arbitrary constant,

$$\delta \int (y + a \sqrt{1 + p^2}) \, dx = 0.$$

Putting V for the quantity in brackets, we have

$$N = \frac{dV}{dy} = 1, \text{ and } P = \frac{dV}{dp} = \frac{ap}{\sqrt{1 + p^2}}.$$

Hence

$$N - \left(\frac{dP}{dx} \right) = 1 - \frac{d}{dx} \cdot \frac{ap}{\sqrt{1 + p^2}} = \Lambda.$$

Consequently $\Lambda \, dx = dx - d \cdot \dfrac{ap}{\sqrt{1 + p^2}} = 0$; and by integrating,

$$x + c = \frac{ap}{\sqrt{1 + p^2}}.$$

Hence, by integrating again,

$$(x + c)^2 + (y + c')^2 = a^2.$$

Also

$$\Lambda p \, dx = p \, dx - p \cdot d \cdot \frac{ap}{\sqrt{1 + p^2}} = dy - \frac{ap \, dp}{(1 + p^2)^{\frac{3}{2}}} = 0;$$

$$\therefore \quad y + c' = -\frac{a}{\sqrt{1 + p^2}};$$

and integrating again,

$$(x + c)^2 + (y + c')^2 = a^2.$$

Thus in this example $\mathrm{A}dx$ and $\mathrm{A}pdx$ are both exact differentials, and the integrations give the same result, viz. the equation of a circle the radius of which is a, and the coordinates of the centre $-c$ and $-c'$. These three arbitrary quantities are always determinable from the given conditions, provided the given length be less than the straight line joining the two points. Hence in *all* cases the required line is an arc of a circle terminating at the given points.

Mr. Todhunter states (art. 366) that this problem is discussed by Legendre, and also by Stegmann in a work published no longer ago than 1854, and that these two geometers arrive at the same result, viz. that the required line is in some cases a circular arc, and in other cases is composed of a circular arc and one or two straight lines. The foregoing solution proves that this conclusion is erroneous. Mr. Todhunter has failed to notice that the error is due to not recognizing the principle of integrating with respect to consecutive points of a curve.

Problem II. Required the minimum surface generated by the revolution of a line joining two given points in a plane passing through the axis of revolution.

According to the principles already laid down, the surface is $\int 2\pi y\, ds$, or $\int 2\pi \sqrt{1 + p^2}\, dx$, whatever be the form of the line, and wherever in the plane the given points be situated, if the integration be supposed to be taken along consecutive points of the line. Hence, by the Calculus of Variations,

$$\delta \int y \sqrt{1 + p^2}\, dx = 0,$$

and $\mathrm{V} = y \sqrt{1 + p^2}$. Consequently

$$\mathrm{N} = \sqrt{1 + p^2}, \quad \mathrm{P} = \frac{yp}{\sqrt{1 + p^2}}, \text{ and } \left(\frac{d\mathrm{P}}{dx}\right) = \frac{p^2}{\sqrt{1 + p^2}} + \frac{yq}{(1 + p^2)^{\frac{3}{2}}}.$$

Therefore

$$\mathrm{N} - \left(\frac{d\mathrm{P}}{dx}\right) = \frac{1}{\sqrt{1 + p^2}} - \frac{yq}{(1 + p^2)^{\frac{3}{2}}} = \mathrm{A};$$

and

$$\mathrm{A}pdx = \frac{dy}{\sqrt{1 + p^2}} - \frac{yp\, dp}{(1 + p^2)^{\frac{3}{2}}} = 0.$$

Hence, by integrating,

$$y = c\sqrt{1 + p^2},$$

which, as is known, is the differential equation of a *catenary* having its directrix coincident with the axis of revolution. It

would seem, therefore, to follow that the condition of a minimum can always be satisfied by a catenary so situated. But on trial this is not found to be the case. Mr. Todhunter states ('Hist. of Calc. of Var.' art. 308) that this problem was the subject of an essay by Goldschmidt which obtained a prize in 1831, in which "the conclusion is, that sometimes two such catenaries can be drawn, sometimes only one, and sometimes no catenary." The same problem is discussed at considerable length in arts. 102–105 of the *Leçons de Calcul des Variations* by MM. Lindelöf and l'Abbé Moigno, and a similar conclusion is arrived at. In fact, this appears to be a *discontinuous* solution of the problem.

Yet it is certain that there can always be drawn between the given points *continuous* curves which, by the revolution about the axis, generate surfaces of *different* magnitudes; and as the descending gradations of magnitude cannot go on indefinitely, there must be a limiting minimum surface, the form of which should admit of being determined by the Calculus of Variations. The purpose of the following investigation is to show how this may be done.

First, it may be remarked that the present problem differs from Problem I. in the respect that $A dx$ is not, as well as $Apdx$, an exact differential. This is one reason for concluding that $Ap=0$ and $A=0$ may not be used indifferently as if they were equivalent equations. Another reason is, that the above first integral of $Ap=0$ does not satisfy $A=0$ and $Ap=0$ in the same manner. For by substituting $c\sqrt{1+p^2}$ for y in the latter equation, we have

$$-\frac{p}{\sqrt{1+p^2}} - \frac{pqc}{1+p^2} = 0,$$

which is satisfied if $p=0$, but not if $p=$ infinity; and by substituting the same value of y in $A=0$, we get

$$\frac{1}{\sqrt{1+p^2}} - \frac{qc}{1+p^2} = 0,$$

which is satisfied if $p=$ infinity, but not if $p=0$, the value of c being arbitrary. These considerations point to the conclusion that the solution of the problem can be effected only by taking into account an *independent* integration of $A=0$.

Such an integration I proceed now to obtain by a method which, as far as I know, is new. The method consists essentially in first finding, when it is possible, the *evolute* of the curve or curves of which $A=0$ is the differential equation, and then employing the involutes thence derivable, which may be regarded as the solution of the equation, to satisfy, either by computation or by graphical construction, the given conditions of the problem.

Let x', y' be the coordinates of that point of the evolute which is the centre of the curvature at the point of the involute whose coordinates are x, y. Then we have the known equations

$$y - y' = -\frac{1+p^2}{q}, \quad x - x' = -p(y - y').$$

Since the equation $A = 0$ gives $1 + p^2 = qy$, it follows that $y' = 2y$, and $x - x' = py$. Consequently, because $p = -\dfrac{dx'}{dy'}$, we obtain

$$x = x' - \frac{y'}{2} \cdot \frac{dx'}{dy'}.$$

Therefore

$$\frac{dy}{dx} = \frac{dy'}{2d.\left(x' - \dfrac{y'}{2}\dfrac{dx'}{dy'}\right)} = -\frac{dx'}{dy'}.$$

Hence it will be readily found, putting p' for $\dfrac{dy'}{dx'}$, that

$$\frac{dy'}{y'} + \frac{dp'}{p'(1+p'^2)} = 0,$$

which equation gives, by integrating,

$$dx' = \pm dy'\left(\frac{y'^2}{k^2} - 1\right)^{\frac{1}{2}},$$

k being the arbitrary constant. Consequently, by integrating again,

$$x' + c' = \pm\frac{y'}{2k}\left(\frac{y'^2}{k^2} - 1\right)^{\frac{1}{2}} - \frac{k}{2}\log\left(\frac{y'}{k} \pm \left(\frac{y'^2}{k^2} - 1\right)^{\frac{1}{2}}\right), \quad . \quad (\alpha)$$

which is the equation of the evolute. From this equation the differential equation of the involutes may be found as follows:—

$$\pm\left(\frac{y'^2}{k^2} - 1\right)^{\frac{1}{2}} = \frac{dx'}{dy'} = -\frac{dy}{dx} = -p.$$

Hence

$$\frac{y'}{k} = \sqrt{1+p^2}.$$

Consequently, since $x' = x + p(y - y') = x + p(y - k\sqrt{1+p^2})$, we get by substituting in the equation of the evolute,

$$x + c' = -py + \frac{kp}{2}\sqrt{1+p^2} + \frac{k}{2}\log(\sqrt{1+p^2} + p). \quad . \quad (\beta)$$

This is the required equation, which, as might be inferred from the reasoning, embraces *all* the involutes of the curve whose equation is (α). It will, however, be proper to substantiate this inference by independent reasoning; which I now propose to do.

First, it is to be remarked that if, by the process employed above, we had deduced the equation of an evolute from $\Lambda p = 0$, we should equally have been conducted to the equation (α), because the factor p would have made no difference.   But in this case, since $\Lambda p = 0$ is at once integrable, we know that there is but *one* involute, namely the catenary whose equation, as obtained by the integration of $y = c \sqrt{1 + p^2}$, is

$$x + c' = c \log\left(\frac{y}{c} + \sqrt{\frac{y^2}{c^2} - 1}\right).$$

By substituting $\sqrt{1 + p^2}$ for $\frac{y}{c}$, this equation takes the form

$$x + c' = c \log(\sqrt{1 + p^2} + p).$$

Now, on comparing this result with (β), it will be seen that the two equations are identical if $\frac{k}{2} = c$ and $-py + \frac{kp}{2}\sqrt{1 + p^2} = 0$. But these equalities give at once the foregoing equation

$$y = c \sqrt{1 + p^2} ;$$

which shows that the integral of $\Lambda p = 0$ is one of the evolutes embraced by the equation (β). This being the case, it is easy to deduce from that integral a differential equation embracing all the evolutes ; which, if our reasoning be good, ought to coincide with the equation (β). I proceed to verify this conclusion.

Let x_i, y_i be the coordinates of any point of the catenary whose equation, as obtained by integrating $\Lambda p = 0$, is

$$x_i + c' = c \log\left(\frac{y}{c} + \sqrt{\frac{y^2}{c^2} - 1}\right) = c \log(\sqrt{1 + p_i^2} + p_i).$$

Then if x, y be the coordinates of the point of intersection of any other involute by the radius of curvature of the catenary at the point $x_i y_i$, or by its prolongation, and if h be the constant interval between the two curves, we shall have

$$x = x_i + \frac{hp_i}{\sqrt{1 + p_i^2}}, \text{ and } y = y_i - \frac{h}{\sqrt{1 + p_i^2}}.$$

Also, since $y = c\sqrt{1 + p_i^2}$, it follows that $h = c(1 + p_i^2) - y\sqrt{1 + p_i^2}$. Hence

$$x_i + c' = x + c' - \frac{p_i}{\sqrt{1 + p_i^2}}\left(c(1 + p_i^2) - y\sqrt{1 + p_i^2}\right)$$

$$= x + c - cp_i\sqrt{1 + p_i^2} + p_i y.$$

Consequently by substitution, since $p_i = p$,

$$x + c' = cp\sqrt{1+p^2} - py + c\log(\sqrt{1+p^2} + p),$$

which equation is identical with (β), it having been already shown that $\dfrac{k}{2} = c$.

If the equation (β) were integrated, an additional parameter would be introduced; and from the foregoing reasoning it follows that, by giving different values to this parameter, c and c' remaining constant, the equations of all the involutes might be obtained. But here an analytical circumstance must be mentioned the signification of which will require particular consideration. The complete integral of the equation (β) will contain *three* arbitrary constants, and therefore cannot be the integral of $A = 0$, which is of the second order. It can, however, be shown as follows that the differential equation resulting from the elimination of the three constants is verified by the equations $A = 0$ and $\dfrac{dA}{dx} = 0$, the latter of which is a true analytical consequence of the other.

For by differentiating the equation (β) to get rid of the arbitrary constant c', we obtain

$$0 = 1 + p^2 + yq - kq\sqrt{1+p^2},$$

or

$$k = \frac{\sqrt{1+p^2}}{q} + \frac{y}{\sqrt{1+p^2}}.$$

Differentiating the last equation to eliminate k, the result is

$$0 = \frac{2p}{\sqrt{1+p^2}} - \frac{r\sqrt{1+p^2}}{q^2} - \frac{ypq}{(1+p^2)^{\frac{3}{2}}}, \quad . \quad . \quad (\gamma)$$

r being put for $\dfrac{dq}{dx}$. If now the value of q be deduced from the equation $A = 0$, and the value of r from $\dfrac{dA}{dx} = 0$ ($A = 0$ being taken into account), it will be found that

$$q = \frac{1+p^2}{y}, \text{ and } r = \frac{pq^2}{1+p^2}.$$

But these values of q and r, being substituted in the equation (γ), cause it to vanish. The inference to be drawn from this result is, that the integral from which the equation (γ) was deduced, although it is incapable of *directly* satisfying the equation $A = 0$, may still be regarded as a solution of that equation, inasmuch as

we have shown that the equation (γ) may be verified by means of the equation $A = 0$ and its derived equation $\dfrac{dA}{dx} = 0$. That integral may consequently be applied in the solution of the proposed problem.

What is gained by this process is, that an additional arbitrary constant is available for satisfying the given conditions of the problem. The integral of $Ap = 0$ fails to give the *continuous* solution because it contains only two arbitrary constants. The required curve might either be described by first constructing the evolute from its equation and then unwinding from it a cord of arbitrary length, or by constructing the catenary given by the equation $Ap = 0$ and then drawing a curve distant from the catenary at all points by the same arbitrary interval. As I do not think it necessary to enter at present more into detail respecting the application of the new integration to this instance, I shall now proceed to apply it to another problem.

Problem III. Required the maximum solid of revolution of given superficies, the generating line of the surface being supposed to join any two given points in a plane passing through the axis of revolution.

In the usual enunciation of this problem the two points are supposed to be in the axis of revolution. But according to the principle of integrating along consecutive points of the curve, as stated in the Lemmas at the beginning of this communication, the process for finding the required differential equation is the same wherever the given points be situated, the coordinates of their positions coming under consideration only in determining the limits of the integration. This problem has hitherto not been generally solved even for the case in which the given points are on the axis. I propose to apply to it a treatment precisely analogous to that employed in the preceding problem.

We have in this example to find a relation between x and y which shall satisfy the condition

$$\delta \int (y^2 - 2ay\sqrt{1 + p^2})\,dx = 0,$$

$-2a$ being an arbitrary constant taken for convenience with a negative sign. By the usual rules of the Calculus of Variations,

$$A = y - \frac{a}{\sqrt{1 + p^2}} + \frac{ayq}{(1 + p^2)^{\frac{3}{2}}}, \text{ and } (\delta y - p\delta x)A = 0.$$

Hence $A = 0$, and by consequence $Ap = 0$. I shall not have occasion to refer to the discontinuous solution which mathematicians have elicited from the latter equation, my sole object being to discover a solution by a *continuous* line joining the two points,

which, from considerations analogous to those adduced in the preceding problem, may certainly be pronounced to be possible.

First it is to be remarked that here, as in the last problem, $A p dx$ is an exact differential, while $A dx$ is not, and consequently that the equations $A p = 0$ and $A = 0$, not being equivalent, must both be taken into account. From the former we have

$$A p dx = y dy - \frac{a dy}{\sqrt{1+p^2}} + \frac{a y p dp}{(1+p^2)^{\frac{3}{2}}} = 0.$$

Hence, by integration,

$$y^2 = \frac{2ay}{\sqrt{1+p^2}} + b^2,$$

b^2 being the arbitrary constant. Consequently

$$dx = \frac{(y^2 - b^2) dy}{\left(4a^2 y^2 - (y^2 - b^2)^2\right)^{\frac{1}{2}}}.$$

This equation cannot be exactly integrated; but MM. Delaunay and Sturm have proved (*Journal de Liouville*, vol. vi. p. 315) that the curve it represents may be described by the focus of an hyperbola which rolls on a straight line. Its form can therefore be ascertained without difficulty. But on doing this it is found that the curve is incapable of giving a solution of our problem, whether or not the given points are on the axis. It has, however, been shown by Mr. Todhunter (Hist. of Calc. of Var. p. 410), for the case in which the problem is to find the greatest solid of revolution of given superficies, the generating line of the surface consisting of ordinates perpendicular to the axis at two given points and of a curve connecting them, that the form of the curve is given by the integral of the above equation, and that the two ordinates are equal and join on to the curve continuously. But the general problem under consideration requires for its solution an independent integration of $A = 0$.

In order to effect such an integration, I shall proceed, just as in Problem II., to deduce from $A = 0$ the equation of the evolute. Putting ρ for the radius of curvature, the equation $A = 0$ gives

$$\frac{1}{\sqrt{1+p^2}} = y\left(\frac{1}{a} - \frac{1}{\rho}\right).$$

Also

$$y - y' = -\frac{1+p^2}{q} = \frac{\rho}{\sqrt{1+p^2}} = \rho y\left(\frac{1}{a} - \frac{1}{\rho}\right).$$

Consequently

$$y = \frac{a y'}{2a - \rho}, \text{ and } y - y' = \frac{y'(\rho - a)}{2a - \rho}.$$

Again,

$$x = x' - p(y - y') = x' - \frac{y'}{p'} + \frac{y}{p'},$$

because $pp' + 1 = 0$.   Hence

$$dx = -\frac{dp'}{p'^2}(y - y') + \frac{dy}{p'} = -\frac{dp'}{p'^2} \cdot \frac{y'(\rho - a)}{2a - \rho} + \frac{dy}{p'}.$$

But

$$\frac{dp'}{v'^2} = d\rho = -\frac{(1 + p'^2)^{\frac{3}{2}}}{\rho} dx;$$

and

$$(1 + p^2)^{\frac{3}{2}} = \left(\frac{\rho}{y - y'}\right)^3 = \frac{\rho^3 (2a - \rho)^3}{y'^3 (\rho - a)^3}.$$

Consequently, by substituting,

$$dx = \frac{\rho^2}{y'^2} \cdot \frac{(2a - \rho)^2}{(\rho - a)^2} dx + \frac{dy}{p'}.$$

Now

$$dx - \frac{dy}{p'} = \left(1 + \frac{1}{p'^2}\right)dx = \frac{d\rho^2}{dy'^2} dx,$$

because $d\rho^2 = dx'^2 + dy'^2$.   Hence it will be readily seen that

$$\frac{dy'}{y'} = \frac{(\rho - a) d\rho}{\rho(2a - \rho)}.$$

This equation gives, by integration, $\rho^2 - 2a\rho = -ky'^2$, k being an arbitrary constant; and by solving the quadratic,

$$\rho = a \pm \sqrt{a^2 - ky'^2}.$$

Then, since

$$d\rho = \sqrt{dx'^2 + dy'^2} = \frac{\mp ky' dy'}{\sqrt{a^2 - ky'^2}},$$

it follows that

$$dx' = \pm dy' \left\{ \frac{(k^2 + k)y'^2 - a^2}{a^2 - ky'^2} \right\}^{\frac{1}{2}}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (\delta)$$

which is the differential equation of the evolute.   If b and c be respectively the values of y' which satisfy the equations $a^2 - ky'^2 = 0$ and $(k^2 + k)y'^2 - a^2 = 0$, the equation may be put under the form

$$dx' = \pm \frac{b \, dy'}{c} \cdot \left(\frac{y'^2 - c^2}{b^2 - y'^2}\right)^{\frac{1}{2}}.$$

Hence it is evident that the values of y' all lie either between $+b$ and $+c$, or between $-b$ and $-c$.   The least value c of y' belongs to a *cusp*, because it makes $\frac{dy}{dx'}$ infinite; also the curve

is symmetrical about the maximum ordinate b. In short, the form of the curve resembles that of a cycloid at a certain distance from the axis of abscissæ. If we put e^2 for $1 - \dfrac{c^2}{b^2}$, and $e^2 \sin^2 \phi$ for $1 - \dfrac{c^2}{y'^2}$, the equation is transformable into

$$dx' = \pm \frac{ce^2 \sin^2 \phi \, d\phi}{(1 - e^2 \sin^2 \phi)^{\frac{3}{2}}},$$

which may be integrated by elliptic functions. Thus it is always possible to describe the evolute. It remains to be shown that the curve which solves the proposed problem is one of its involutes.

Resuming the equation (δ), and substituting in it p^2 for $\dfrac{dx'^2}{dy'^2}$, and for y' its value $\dfrac{y}{a}(2a - \rho)$, we obtain

$$p^2 = \frac{(k^2 + k)\dfrac{y^2}{a^2}(2a - \rho)^2 - a^2}{a^2 - k\dfrac{y^2}{a^2}(2a - \rho)^2}.$$

As this equation contains ρ, it is of the second order, and its integral will involve two arbitrary constants in addition to k. That integral cannot, therefore, satisfy $A = 0$, which is of the second order. But it is important to remark that the equation obtained by eliminating the *three* arbitrary constants is satisfied by the values of q and r deduced respectively from $A = 0$ and $\dfrac{dA}{dx} = 0$. It is evident that the equation resulting from such elimination is obtained by simply eliminating k by differentiation from the foregoing equation; which is to be done as follows.

By solving the equation as a quadratic with respect to k, and putting for the sake of shortness P^2 for $1 + p^2$, it will be seen that

$$k = -\frac{P^2}{2} \pm \left(\frac{P^2 a^4}{y^2 (2a - \rho)^2} + \frac{P^4}{4} \right)^{\frac{1}{2}},$$

which, since $P = \dfrac{a\rho}{y(\rho - a)}$, may be put under the form

$$k = -\frac{a^2 \rho^2}{2y^2 (\rho - a)^2} \left\{ 1 \mp \left(\frac{4a^2 (\rho - a)^2}{(2a - \rho)^2} + 1 \right)^{\frac{1}{2}} \right\}.$$

If this equation be differentiated to get rid of k, the result will be a complicated equation containing y, p, ρ, and $\dfrac{d\rho}{dx}$. Now I

have found by a somewhat long process, which it would be tedious to give in detail here, that by substituting for ρ and $\dfrac{d\rho}{dx}$ from the equations $A = 0$ and $\dfrac{dA}{dx} = 0$, that equation is satisfied. It is to be noticed that it is not satisfied if $A = a$ constant; so that this equation of the third order is not identical with $\dfrac{dA}{dx} = 0$.

Since it is necessary to employ both $A = 0$ and $\dfrac{dA}{dx} = 0$ for its verification, the integral from which it was derived may be considered to be a solution of the former equation, and therefore proper for being applied in the present problem.

The same equation (δ) of the evolute would have been obtained if we had employed $Ap = 0$ instead of $A = 0$. Hence among the involutes obtained must be included the particular one resulting from the integration of the former equation. In fact, the process may be so conducted as to lead exclusively to that involute. For this purpose it suffices to substitute for $2a - \rho$, in the foregoing expression for p^2, its value $\dfrac{a(Py - 2a)}{Py - a}$, so as to obtain the result

$$1 + p^2 = P^2 = \frac{k^2 y^2 (Py - 2a)^2}{a^2 (Py - a)^2 - ky^2 (Py - 2a)^2},$$

which only differs in form from the first integral of $Ap = 0$.

Just as in the solution of Problem II., the required curve may be described either by unwinding a cord from the evolute given by the equation (δ), or by drawing a curve equidistant at all points from that given by the integration of $Ap = 0$. The similarity of the processes in the two instances is some evidence of the correctness in principle of the new method of integrating. I have not leisure to discuss in more detail the solution of Problem III., and shall only add that when the two given points are on the axis, if the length of the curve be not much greater than the distance between them, the solution shows that its form is like the arc of a bow; but if much greater, that the form approaches that of a circle.

I think I may say that I have at length succeeded, after repeated failures, in removing from analytics the reproach of being incapable of solving this class of problems. Perhaps the peculiarity of the requisite process may account for its being so long undiscovered.

Cambridge, June 17, 1871.

IV. *An Inquiry into the Cause of the Interrupted Spectra of Gases.*—Part II. *On the Absorption-spectrum of Chlorochromic Anhydride.* By G. Johnstone Stoney, *M.A.*, *F.R.S.*, *Secretary to the Queen's University in Ireland,* and J. Emerson Reynolds, *M.R.C.P.E., Keeper of the Mineral Department, and Analyst to the Royal Dublin Society*.*

<div align="center">Contents.</div>

Section I. *Introductory.*

1. ONE of the authors of this communication endeavoured[†] in 1867 to call attention to the circumstance that wherever the spectrum of a gas consists of lines of definite wavelengths there must be *periodic* motions in the gas, and that motions of this kind can exist only within the individual molecules of the gas; and more recently[‡] he has pointed out that from each periodic motion there will usually arise several lines, and that the lines which thus result from one motion will have periods that are harmonics of the periodic time of the parent motion.

2. In our endeavours to bring this theory and its various consequences to the test of observation, we commenced with absorption-spectra, for the examination of which the apparatus at our immediate command was best suited. The apparatus consisted mainly of the great Grubb spectroscope of the Royal Dublin Society, and of the appliances in the laboratory of the Society for keeping up an abundant supply of the oxhydrogen lime light.

3. The chief obstacles we anticipated were those that arise from the extreme closeness with which the lines are often found to be ruled, and those to be expected from the complexity of spectra; for usually lines belonging to several distinct systems are presented together to the eye within the same field of view, and this makes an apparently confused maze of lines, from which it is difficult to pick out those that are to be referred to any one motion in the gas. Accordingly, as a preliminary step,

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* Communicated by the Authors, having been read before the Royal Irish Academy, June 12, 1871.

† Phil. Mag. vol. xxxvi. (1868) p. 132.

‡ Phil. Mag. vol. xli. (1871) p. 291, and Proceedings of the Royal Irish Academy of January 9, 1871. Report of the British Association for 1870, p. 41.

we looked at several of the absorption-spectra of coloured vapours, to see whether amongst them we could find one in which there is a system of lines which we might hope to refer to a single motion in the molecules of the vapour, free from lines emanating from other motions in the molecules of the vapour, and sufficiently separated from one another to be easily measured. A few days after we commenced operations we were so fortunate as to meet with the object of our search. The brown vapours of chlorochromic anhydride ($Cr O^2 Cl^2$) when interposed between the lime-light and the spectroscope gave a spectrum of the requisite simplicity.

4. In order to test that part of the theory which indicates that the periodic times of the wave-vibrations of the several lines are harmonics of one periodic time, we find it convenient to refer the positions of all lines to a scale of reciprocals of the wave-lengths. This scale of inverse wave-lengths has the great convenience for our present purpose, that a system of lines with periodic times that are harmonics of one periodic time will be equidistant upon it: it has also the minor convenience that it much more closely resembles the spectrum, as seen, than the scale of direct wave-lengths used by Ångström in his classic map. Upon our scale the inverse wave-length 2000 corresponds to Ångström's direct wave-length 5000. The numbers which Ångström uses are tenth-metres, *i. e.* the lengths obtained by dividing the metre into 10^{10} parts; and from this it follows that each number upon our scale is the number of light-waves in a millimetre: thus 2000 upon our scale means that the corresponding wave-length is $\frac{1}{2000}$ of a millimetre. Now, if k be the inverse wave-length, expressed upon our scale, of a fundamental motion in the æther, its direct wave-length will be $\frac{1}{k}$th of a milli-metre, and its harmonics will have the wave-lengths $\frac{1}{2k}$, $\frac{1}{3k}$, &c. Accordingly the inverse wave-length of the nth harmonic will be

$$i_n = (n+1) \cdot k. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Hence it is easy to see that a system of harmonics which are equally spaced along our scale at intervals of k divisions are harmonics of a fundamental motion whose inverse wave-length is k, whose direct wave-length is $\frac{1}{k}$th of a millimetre, and whose periodic time is $\frac{\tau}{k}$, where τ is the periodic time of an undulation in the æther consisting of waves one millimetre long. If we use

Foucault's determination of the velocity of light, viz. 298,000,000 metres per second, the value of this constant is

$$\tau = 3\cdot3557 \text{ twelfth-seconds,} \quad . \quad . \quad . \quad (2)$$

meaning by a twelfth-second a second of time divided by 10^{12}, which in other words is the millionth part of the millionth of a second of time.

Section II. *On the periodic time of one of the motions in the molecules of Chlorochromic Anhydride.*

5. We generally made use of the vapour of chlorochromic anhydride mixed with air, and at the temperature and pressure of the atmosphere. We tried the vapour freed from air, and also at somewhat higher temperatures; but in neither case did we observe any marked difference in the spectrum. An image of the lime-light was formed on the slit of the spectroscope by a condensing-lens of about 30 centims. focus, and of such a size that the whole of the collimating-lens was filled with light[*]. The column of chlorochromic vapour, if not too long[†], was placed between the condensing-lens and the slit. The spectrum which then presents itself consists of a number of sensibly equidistant dark lines in the orange, yellow, and green. In the orange these lines fade away and leave part of the orange and the red unsubdued by lines. In the other direction the lines are gradually lost in an increasing obscuration of the spectrum, which entirely blots out the more refrangible colours. The lines are nowhere sharply defined or narrow, nor are the spaces between them devoid of duskiness[‡]; and the region of general absorption, which occupies the more refrangible part of the spec-

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[*] This is a condition which is essential to making good measures. If only a vertical strip of the collimating-lens is supplied with light, the beam belonging to one line in the spectrum reaches the image in the observing-telescope as a thin wedge of light, upon which the eye does not readily focus itself. Under these circumstances the eye keeps continually altering its adjustment, feeling about for the right distance; and if the strip of light has not passed exactly centrally through the instrument, this causes the image of the line to appear to deviate in various degrees from its true position, accordingly as the adjustment of the eye fluctuates.

[†] We used columns of various lengths, varying from 4 to 80 centims. From 15 to 20 centims. is a good average length. By increasing the length, the lines in the yellow and orange become more conspicuous; and by diminishing it the lines in the green are better seen. With the longest column we were obliged to place two condensing-lenses at the ends of the tube, one to send the light from the lime-light in a parallel beam along the tube, and the other to condense this parallel beam into an image upon the slit of the spectroscope.

[‡] See further on, § 22.

trum, seems to consist simply of lines of the same series so widened out that they are blended into one mass.

6. For the convenience of reference we have numbered the lines from a conspicuous one which happens to fall between the two D lines, nearer to the more refrangible one. There are 106 lines, counting from this line inclusive to a point somewhat beyond *b*; and we have measured the deviations of 31 of these in a spectroscope giving a dispersion from A to H of about 24°. We have from these measures deduced the inverse wave-lengths by a comparison with the deviations of forty lines of iron, copper, zinc*, and sodium, extending over the same range of the spectrum, of which the wave-lengths in air are recorded by Angström and Thalen. The interpolation has been effected by a graphical method; and our measures in no case, when repeated, differed by one minute of arc†.

7. The first column of the following Table gives the numbers of the several lines which we measured, reckoned from that line which lies between the two D lines. The second column gives the observed positions of the lines upon a scale of inverse wavelengths in air, viz. upon a scale of the reciprocals of Ångström's wave-lengths, which are wave-lengths in air of standard pressure and 14° temperature. The third column contains the corresponding positions upon a scale of inverse wave-lengths *in vacuo*, obtained by applying to the numbers of column 2 the corrections for the dispersion of air at 760 millims. pressure and 14° temperature, deduced from Ketteler's values‡. The fourth column gives the calculated positions, on the hypothesis that the lines of the spectrum are equally spaced upon this last scale, as they should be according to our theory. And the fifth column gives the differences between columns 4 and 3, between the calculated and observed positions.

* The zinc-line to which Thalen assigns the wave-length 5745, appears rather to have a wave-length of about 5739.

† The measuring-apparatus of the spectroscope has only recently been completed. It is apparent to us that the instrument is capable of measuring deviations even far more accurately than we have yet attempted. This is due to the extraordinary precision with which Mr. Grubb's automatic arrangement returns over and over again to the same position. It has, indeed, almost made of the spectroscope a new physical instrument.

‡ See Phil. Mag. vol. xxxii. (1866) p. 336.

TABLE I.—Positions of 31 lines of the absorption-spectrum of Chlorochromic Anhydride.

No. of line.	Observed positions in air.	Corresponding positions in vacuo.	Calculated positions in vacuo.	Differences.	No. of line.	Observed positions in air.	Corresponding positions in vacuo.	Calculated positions in vacuo.	Differences.
0.	1697·5	1697·0	1697·3	−0·3	60.	1860·0	1859·5	1859·3	+0·2
5.	1711·7	1711·2	1710·8	+0·4	61.	1862·0	1861·5	1862·0	−0·5
10.	1724·5	1724·0	1724·3	−0·3	63.	1867 8	1867·3	1867·4	−0·1
15.	1738·7	1738·2	1737·8	+0·4	66.	1876·1	1875·6	1875·5	+0·1
20.	1752·2	1751·7	1751·3	+0·4	68.	1881·3	1880·8	1880·9	−0·1
25.	1766·0	1765·5	1764·8	+0·7	71.	1889·7	1889·2	1889·0	+0·2
28.	1774·0	1773·5	1772·9	+0·6	74.	1897·9	1897·4	1897·1	+0·3
30.	1779·1	1778·6	1778·3	+0 3	76.	1902·6	1902·1	1902·5	−0·4
33.	1787·1	1786·6	1786·4	+0·2	79.	1911·5	1911·0	1910·6	+0·4
36.	1794·1	1793·6	1794·5	−0·9	81.	1916·2	1915·7	1916·0	−0·3
40.	1805·5	1805·0	1805·3	−0·3	84.	1924·5	1924·0	1924·1	−0·1
45.	1818·9	1818·4	1818·8	−0·4	89.	1937·5	1937·0	1937·6	−0·6
50.	1832·4	1831·9	1832·3	−0·4	92.	1946·5	1946·0	1945·7	+0·3
53.	1840·2	1839·7	1840·4	−0·7	97.	1960·4	1959·8	1959·2	+0·6
55.	1845·5	1845·0	1845·8	−0·8	105.	1980·7	1980·1	1980·8	−0·7
58.	1853·7	1853·2	1853·9	−0·7					

The outstanding differences fall within the limits of the errors of observation and interpolation. Our theory is therefore verified in the case of chlorochromic anhydride.

8. The interval between two consecutive lines, which we used in calculating column 4, was 2·70 scaleins, or units of our scale. This value cannot be in error more than one five-hundredth part. Hence the periodic time of the parent motion in the molecules of chlorochromic anhydride, from which all these lines have proceeded, is within one five-hundredth part of $\frac{\tau}{2·7}$, τ having the signification already assigned to it.

9. Having ascertained k, or the interval between two consecutive lines, we may by equation (1) determine n, or the number of the harmonic. Thus, if the inverse wave-length of our zero-line be 1697·3, and if k be nearly 2·7, $n + 1$ must be an integer which is nearly $= \frac{1697·3}{2·7} = 628·6$. The only possible values are 627, 628, 629, and 630, since any other integers would carry us beyond the errors of observation. And when the measures shall have been made with sufficient accuracy to decide which of these numbers is the true one, it will in turn be possible to fix the value of k with great precision. Thus, if, as is most probable, our zero-line is the 628th harmonic, and if its inverse wave-length *in vacuo* is 1697·3, then by equation (1) will

$$k = \frac{1697·3}{629} = 2·6984, \quad . \quad . \quad . \quad . \quad (3)$$

which we offer as probably a very close approximation to the value of k.

SECTION III. *On the character of this molecular motion.*

10. But beside thus determining with very great precision the periodic time of one of the motions in the molecules of the vapour of chlorochromic anhydride, the study of this spectrum has elicited other information about the motion, which we think ourselves justified in putting upon record, although it is imperfect.

11. It was just before leaving off work on the 28th of January that we first looked at the absorption-spectrum of chlorochromic anhydride, and found that it gives in the greenish yellow a spectrum of lines arranged in nearly the following pattern,

 . . . (4)

each section of the pattern consisting of five lines, a very dark one followed by a very light one, then two of medium intensity, and then another very light one.

12. We saw no more upon that evening; but in thinking over the pattern afterwards, it seemed to offer some hope that we should be able to trace out not only the periodic time of the parent motion to which the lines are due, but even some information regarding the character of that motion. In fact the pattern seemed to suggest a very simple law of variation of intensity in passing from line to line, viz. that the intensities may probably be related in some simple way to the lengths of the ordinates of a curve of sines raised from points which divide the interval from crest to crest into five equal parts, thus

 . (5)
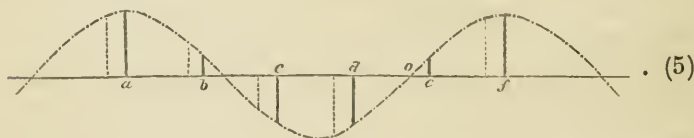
For example, taking the expression for the displacement-curve of the original disturbance in the æther, which is[*]

$$y = A_0 + C_1 \sin (x + \alpha_1) + C_2 \sin (2x + a_2) + \ldots, \quad . \quad . \quad (6)$$

the requisite condition would be fulfilled in the simplest manner, if six successive coefficients (the squares of which represent the intensities of six successive lines in the spectrum) had the following values :—

* Phil. Mag. vol. xli. (1871), p. 292.

$$
\left.
\begin{aligned}
C_n &= k \sin \theta, \\
C_{n+1} &= k' \sin \left(\theta + \tfrac{1}{5}\pi\right), \\
C_{n+2} &= k'' \sin \left(\theta + \tfrac{2}{5}\pi\right), \\
C_{n+3} &= k''' \sin \left(\theta + \tfrac{3}{5}\pi\right), \\
C_{n+4} &= k^{\mathrm{iv}} \sin \left(\theta + \tfrac{4}{5}\pi\right), \\
C_{n+5} &= k^{\mathrm{v}} \sin \theta.
\end{aligned}
\right\} \qquad \cdots \cdots \quad (7)
$$

where θ is some odd number of times $\dfrac{\pi}{2}$, and the k's are coefficients which only gradually change in passing from line to line.

13. This at once suggested a displacement-curve consisting of a pair of lines repeated over and over, like the sides of the teeth of a saw. For the equation of this displacement-curve is known to be*

$$
y = 2\pi \Sigma \left\{ \frac{\alpha - \beta}{n^2} \cdot \sin n \frac{x_1}{2} \cdot \sin n\left(x - \frac{x_1}{2}\right) \right\}. \quad (8)
$$

And in this equation

$$
C_n = 2\pi \frac{\alpha - \beta}{n^2} \sin n \frac{x_1}{2},
$$

which would assume the required form (5) if

$$
x_1 = \tfrac{4}{5}\pi \pm 2\epsilon, \quad \cdots \cdots \cdots \quad (9)
$$

ϵ being small. The expression for C_n then becomes

$$
C_n = 2\pi \frac{\alpha - \beta}{n^2} \cdot \sin n\left(\frac{2\pi}{5} \pm \epsilon\right),
$$

which would give the observed pattern in those regions of the spectrum in which n has such values as make

$$
n\epsilon \equiv \text{an odd number of times } \frac{\pi}{2}
$$

(using the symbol $\equiv$ to signify *is nearly equal to*). For these parts of the spectrum

$$
C_n \equiv 2\pi \cdot \frac{\alpha - \beta}{n^2} \cos n \frac{2\pi}{5},
$$

which gives the pattern represented in fig. (4).

14. Midway between two such regions

$$
n\epsilon \equiv \text{an even number of times } \frac{\pi}{2},
$$

---

* Equations (8) and (9) taken together represent the motion of a point on a violin-string which is nearly, but not quite, two fifths of the length of the string from one end. See Helmholtz's *Lehre von den Tonempfindungen*, edition 1870, Beilage VI.

which assigns to the corresponding parts of the spectrum the following pattern :—

 . . . . (10)

Halfway between the regions (4) and (10) the pattern would be

 . . (11)
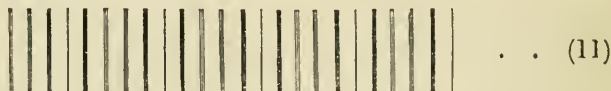
And in general the variation of the pattern in passing along the spectrum may be represented to the eye by conceiving the system of six ordinates in fig. (5) to travel sideways while the curve is fixed. In the position represented by the continuous lines in fig. (5), the squares of the ordinates represent intensities which would give pattern (4). If they are shifted to the left into the position of the dotted lines until e comes to the point of intersection o, e will entirely disappear, a, d, and f will become of equal length, while b and c also become equal but shorter than the other three; and in this position the squares of the lengths of the ordinates will represent the intensities of lines which would give pattern (10). And all other patterns which could arise on this hypothesis would be represented by the other positions of this system of six ordinates.

15. On the 30th of January we were able to compare these anticipations with the spectrum itself; and we had the satisfaction of finding that changes closely approximating to the predicted changes of pattern actually take place in the absorption-spectrum of chlorochromic anhydride, but not at the uniform rate of change which the simple hypothesis represented by equations (8) and (9) would indicate.

16. The results of the comparison are embodied in the following Table, the left-hand side of which gives the observed intensities of the lines on an arbitrary scale in which 10 indicates a very dark line, and 1 the faintest visible; while the right-hand side states what the succession of intensities would be on the hypothesis represented by equations (6) and (7). Notes of interrogation are introduced when the lines were too much dilated for the observation of their intensities. The lines drawn between the two columns point out where the observed succession of intensities of column I. can be most nearly matched in column II. The changes are in the main the same in the two columns; but the rate of change follows a law in column I. which has not yet been traced out, while in column II. it is uniform.

TABLE II.—Intensities of 105 lines of the absorption-spectrum of the vapour of Chlorochromic Anhydride.

From line	Observed intensities.						Calculated intensities.					
−15 to −10 inclusive	4.	1.	2.	2.	0.	3.	4.	8.	0.	8.	4.	4.
−10 to −5 "	5.	1.	2.	3.	0.	4.	5.	7.	1.	9.	3.	5.
−5 to 0 "	7.	3.	3.	4.	1.	5.	6.	6.	2.	10.	2.	6.
0 to 5 "	8.	3.	3.	5.	1.	7.	7.	5.	3.	9.	1.	7.
5 to 10 "	9.	3.	5.	7.	2.	8.	8.	4.	4.	8.	0.	8.
10 to 15 "	10.	3.	5.	6.	2.	9.	9.	3.	5.	7.	1.	9.
15 to 20 "	10.	2.	6.	6.	2.	10.	10.	2.	6.	6.	2.	10.
20 to 25 "	10.	1.	7.	5.	3.	10.	9.	1.	7.	5.	3.	9.
25 to 30 "	10.	0.	9.	4.	4.	10.	8.	0.	8.	4.	4.	8.
30 to 35 "	5.	0.	10.	2.	4.	5.	7.	1.	9.	3.	5.	7.
35 to 40 "	5.	0.	6.	(?)	2.	5.	6.	2.	10.	2.	6.	6.
40 to 45 "	6.	(?)	5.	(?)	(?)	6.	5.	3.	9.	1.	7.	5.
45 to 50 "	7.	(?)	4.	(?)	(?)	7.	4.	4.	8.	0.	8.	4.
50 to 55 "	8.	2.	6.	2.	2.	8.	3.	5.	7.	1.	9.	3.
55 to 60 "	6.	3.	9.	3.	3.	6.	2.	6.	6.	2.	10.	2.
60 to 65 "	4.	3.	9.	2.	6.	4.	1.	7.	5.	3.	9.	1.
65 to 70 "	0.	4.	9.	1.	8.	0.	0.	8.	4.	4.	8.	0.
70 to 75 "	0.	5.	4.	0.	9.	0.	1.	9.	3.	5.	7.	1.
75 to 80 "	0.	7.	0.	1.	9.	0.	2.	10.	2.	6.	6.	2.
80 to 85 "	0.	8.	0.	2.	7.	0.	3.	9.	1.	7.	5.	3.
85 to 89 "	0.	6.	0.	3.	3.	0.	4.	8.	0.	8.	4.	4.

17. We have endeavoured by modifying this simple hypothesis, or substituting another, to gain a closer approach to the actual phenomena, and we have in this way been able to fit the hypothesis to the phenomena; but it has been by assumptions which are as yet too arbitrary to warrant our placing the somewhat complicated details before the public. The simple case given above will sufficiently explain the method we employ; and we think we have received sufficient encouragement from the results of our discussion to hope that this method may elicit in some cases really valuable information about the nature as well as the periodic times of molecular motions.

Section IV. *On the perturbations the motion suffers.*

18. In conclusion we wish to advert to one other phenomenon which appears to us worthy of note. Every line in a spectrum, in order to be visible, must have a certain physical as well as instrumental breadth. By the physical breadth of a line we mean that breadth which it has because the light that constitutes it is not restricted to one wave-length, but extends between certain limits of wave-length; by the instrumental breadth we mean that appearance of breadth which is given to a line by the width of the slit of the spectroscope. A line becomes invisible if either its physical or its instrumental breadth dwindles to zero*. Now the lines of chlorochromic anhydride have a very considerable physical breadth. Hence the original disturbance communicated to the æther by the motion in the vapour consists of waves of corresponding physical breadth. This must be occasioned either by a property inherent in the æther, whereby it can expand over a certain range of wave-length a disturbance which it receives from a strictly isochronous source; or it is due to real differences in the periodic times of the motions in the molecules of the vapour†. Now the variety of the phenomenon in the spectra of different gases forbids our accepting any general explanation, such as that which alleges a property of the æther; and we are therefore compelled to admit that the motions in the molecules of the vapour are not strictly isochronous, but that the periodic times of some of them slightly exceed, and of others fall short of the mean periodic time. The presumption appears to be that the motions within the molecules have naturally a definite pe-

* Phil. Mag. vol. xxxvi. (1868) § 5. p. 136.
† The supposition that it may be attributed to the irregular journeys of the molecules amongst one another, which must in some cases lengthen and in other cases shorten the intervals at which the light-waves reach the eye, is excluded (1) by the amount of the effect (which is beyond what this cause could produce), and (2) by the circumstance that lines situated in the same region of the spectrum are variously expanded.

riodic time, but that this period is exposed to perturbation when two molecules pass sufficiently close to one another, and that during the intervals between two perihelion passages it settles down towards its mean value (Phil. Mag. vol. xxxvi. (1868) p. 135). The perturbations, as a rule, seem to take place in both directions, some of them increasing and others of them di- minishing the periodic time; for in most gases the lines widen out in both directions on raising the temperature—that is, on rendering the molecular collisions more violent. But it some- times happens that the disturbed motion differs in character from the normal motion so sensibly that it gives rise to a some- what different pattern of spectrum.

19. For example, this occurs in the case of the principal lines, the D lines, of the sodium-spectrum. By introducing sodium- carbonate into a suitable part of the oxyhydrogen flame, the bright lines can be made to widen out to any desired amount; while the original positions of the lines are at the same time presented to the eye by the dark absorption-lines, or reversed spectrum as it is called, caused by the surrounding mantle of cooler sodium- vapour. In this way the positions of the lines when narrow and when wide can be directly compared, and will be found to differ. In neither of the lines does the dark line lie in the middle of the bright band: in D_I (the more refrangible one) it inclines towards the red end of the spectrum, and in D_{II} (the less refrangible one) it inclines towards the blue. Hence the middles of the broad bands are further asunder than the narrow lines.

20. A similar appearance is met with in the case of chloro- chromic anhydride. The eye can easily detect that the lines are not everywhere equally spaced, though the deviation of any one line from its calculated position is so slight that in the measures we have taken the amount cannot be separated from errors of observation. The middles of the lines appear to be displaced by small amounts, some to the left, others to the right. And since those parent motions in the molecules of the vapour whose pe- riodic times are somewhat longer than the normal amount must have a predominance in the formation of the lines that deviate towards the blue end of the spectrum, and *vice versâ*, we can by this property distinguish between the lines which belong to mo- tions in the gas that are faster than, equal to, or slower than, those of normal period; and by treating each of the series of lines so obtained as in section III. of this paper, information as to the differences in the nature of the motion in these three cases may perhaps be attainable.

21. It is evident that we only need to obtain more accurate measures to be able, by applying the method described above in § 9, to determine with precision by how much the periodic motion

of each line is in excess or falls short of the mean periodic time.
Thus, if the line we have taken as our zero-line be, as we sup-
pose, the 628th harmonic of the fundamental motion, and if, as
is likely, its inverse wave-length be 1697·0 (the observed amount)
rather than 1697·3 (the calculated amount), it will follow that
this line arises mainly from motions with the period

$$\frac{\tau \times 629}{1697 \cdot 0} = \frac{\tau}{2 \cdot 6979},$$

which is somewhat longer than the duration we have assigned* to
the mean periodic time, viz. $\frac{\tau}{2 \cdot 6984}$. It would thus seem pro-
bable that this line is due principally to those disturbed motions
in the molecules of the vapour which continue sufficiently long
in a phase in which they have a periodic time about 1·0002
times the normal periodic time.

22. To this branch of the subject belongs also that shading
between the lines which is occasioned by still wider departures
from the mean periodic time. This shading evidently follows
laws intimately associated with the laws which determine the
general pattern of the spectrum; for we have noticed that marked
excesses and defects in the shading recur between the corre-
sponding lines of several successive sections of the pattern. But
this is a part of the phenomenon to which we have not yet had
time to give sufficient attention, and we therefore merely men-
tion it here.

Section V. *Conclusion.*

23. We think that our measures satisfactorily confirm the
theory which was recently laid down by one of us as to the
cause of the lines which present themselves in the spectra of
gases, and that we have ascertained with considerable precision
the normal periodic time of one of the motions in the molecules
of the vapour of chlorochromic anhydride at the temperature
and pressure of the atmosphere.

In reference to the other matters upon which we have ven-
tured to touch, the results at which we have as yet arrived
are defective and less secure; and we have entered into them
only so far as appeared necessary to explain the methods we
pursue.

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* See equation (3) above.

V. *On the Direct Conversion of Dynamic Force into Electricity.* *By* S. Tolver Preston, *Esq.**

IT is a well-known fact that electricity is capable of direct conversion into motion, this principle being illustrated in its simplest form by the use of apparatus in which metallic circuits alone are employed, motion being effected, as in the case of the apparatus of Ampère, without the use of either commutator or magnet. As far as I am aware, the exact converse of this case, *i. e.* the direct conversion of dynamic force into electricity without the aid of magnetism, has not been shown to be possible. The apparatus of Faraday, consisting of a metallic disk caused to rotate between the poles of a permanent magnet, is well known—the current thus induced in the disk having been conducted through a galvanometer whose terminals were in connexion with the axis and circumference of disk respectively.

In the annexed figure the helix S takes the place of the galvanometer, one extremity being in connexion with the circumference of disk D through a spring S', the other extremity being connected in any convenient manner with the axis.
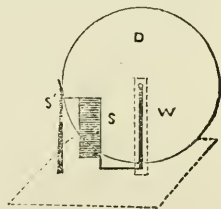
In the apparatus of Faraday the current induced in the disk was solely due to the influence of the permanent magnet, the current traversing that portion of the circuit formed of the galvanometer producing no useful effect. Supposing, however, in the present case the pole of a magnet or a second separate helix traversed by a current (from any source) to be placed near and parallel to the helix S, then the current thus induced by the rotation of the disk will traverse the helix S which forms part of the circuit of the apparatus, and (by a suitable direction of rotation) the direction of the current in both helices will be identical.

The current traversing the exterior circuit is therefore by this arrangement capable of contributing to the effects, both helices being similarly situated in relation to the disk and traversed by a current in the same direction.

Moreover, the helix S forming part of the circuit of the apparatus, the intensity of the current traversing it is therefore a function of the velocity of rotation.

The inductive influence on the disk of the helix S, which depends on the intensity of the current traversing its coils, is therefore capable of indefinitely exceeding that due to the separate helix, in which the intensity of the current, whatever the

* Communicated by the Author.

velocity, can only remain constant (its potential may be supposed indefinitely low).

It is therefore clear that, after the first inductive impulse has been given by the presence of the separate helix, the latter may be removed without appreciably affecting the result, the maintenance of the current being due to the dynamic force employed in effecting rotation, or to the inductive influence upon each other of the portions of the circuit in relative motion.

If a core of soft iron were supposed introduced into the helix S, and on rotating the disk a magnet were only momentarily approached (thus imparting by induction a slight polarity to the iron core), it is clear that the velocity of rotation necessary to maintain a current of given intensity would be much reduced; for the iron core, becoming magnetic under the influence of the current circulating in the surrounding helix, would thus contribute to the effects without adding to the resistance of the circuit, on the amount of which, *cæteris paribus,* the intensity of the current depends.

The following principle serves further to elucidate the subject. On the rotation of any machine by the influence of electricity, the motive current is lowered in intensity by the inductive action of the moving parts of the machine; but by causing the rotation to take place by the use of force in the opposite direction, the intensity of the original current is, conversely, augmented ; or, as explained by the law of the conservation of force, the electric current in effecting the rotation of the machine is reduced in intensity by an amount which is represented by the portion converted into dynamic energy, and on driving the machine in the opposite direction the intensity of the original current is raised by an amount which is the equivalent of the mechanical energy employed.

The apparatus described is in principle clearly an electric motor; *i. e.* a current supposed to traverse the circuit made up of the helix and radius of disk would tend to effect the rotation of the latter ; therefore by the principle just stated, if the apparatus be caused to rotate in a suitable direction, a current introduced into its circuit by induction or otherwise would be raised to an intensity dependent on the dynamic force employed in effecting rotation.

If no limit be assigned to the velocity of rotation which could be imparted, it follows as a logical fact that the circuit could be fused from the relative motion of its parts without friction.

The apparatus consisting in principle of a simple circuit, one portion of which is put in motion, illustrates, as it seems to me, in a striking manner, from its directness and simplicity, the conversion of dynamic energy into those forms of molecular motion termed electricity and heat.

The question suggests itself whether, by improvement of the construction, this principle might not be made practically available in those cases where a uniform current (without successive inversions) is desirable, as afforded by a voltaic battery.

It may be remarked that in the figure a portion of the current finds a circuit within the disk itself; this, however, may be avoided by a change in the details of construction, as is the case with the apparatus of Ampère, which shows the electrodynamic rotation of a current.

The theoretic interest of the question will not, I think, be disputed, the converse principle (that is, the theory of the direct production of motion from electricity) being dealt with in every work professing to contain the principles of the science.

PS.—Since this paper was written I have been informed by Mr. C. F. Varley, to whom the subject treated of was communicated, that to his knowledge this question has been considered mathematically by Sir William Thomson. I am not, however, aware that any paper on the subject has been published. I trust, therefore, that this communication may be found not unsuited for insertion in your valuable pages.

London, June 12, 1871.

VI. *On the Physical Constitution of the Sun.*
By Professor W. A. NORTON[*].

A MONG the recent theories of the Physical Constitution of the Sun, based on the later discoveries, astronomical and spectroscopic, that propounded a few years since by M. Faye[†] has been most favourably received. It is an essential feature of this theory that the sun's mass consists wholly, or in a great degree, of gases or vapours, and that a process of interchange of solar matter between the interior and the photosphere is in incessant operation, in ascending and descending currents, by which the solar radiation is maintained. In a paper by J. Homer Lane " On the Theoretical Temperature of the Sun, &c.," published in Silliman's Journal, July 1869, it is elaborately argued, and appears to be successfully maintained, that the great rapidity of circulation required by this theory cannot subsist consistently with the received laws of gaseous circulation. Quite recently another theory of the sun's physical constitution has been propounded by Professor F. Zöllner, of Leipzig—based mainly on the well-established fact that the solar protuberances, conspicu-

ously visible in total eclipses, and observable at all times with the aid of a spectroscope, are most of them due to violent eruptions of masses of incandescent hydrogen[*]. He remarks that "it is impossible, without passing beyond the well-known analogies necessary for the explanation of cosmical phenomena, to assign any other cause to these eruptions than the difference of pressure of the gases emanating from the interior and from the surface of the sun. To make such a difference of pressure possible, it is necessary to admit the existence of a separating stratum between the inner and outer strata of hydrogen, the latter of which, as is well known, forms an important portion of the solar atmosphere. In reference to the physical constitution of this stratum, we must furthermore assume that it cannot be gaseous, and must therefore be either solid or liquid." He remarks further that "with regard to the inner masses of hydrogen bounded by that stratum two suppositions are possible, viz. :—(1) The whole interior of the sun is filled with incandescent hydrogen gas, which would make the sun an immense bubble of hydrogen surrounded by a liquid glowing envelope. (2) The masses of hydrogen bursting out into protuberances are local collections in bubble-like caverns, which form in the superficial layers of a liquid glowing mass and burst through when the pressure of the confined gas increases." Professor Zöllner adopts the latter supposition as the more probable of the two.

This theory may furnish an adequate supposable cause for the observed eruption of incandescent masses of hydrogen ; but its fundamental hypotheses have no secure ground to rest upon. The notion that the sun's photosphere is in the liquid state is irreconcilable with the astonishing rapidity with which changes often occur on the sun's surface, and also with the fact that the vast elevated masses, seen as the faculæ, occasionally retain the same position for several days—and though suggested long since, has not, to my knowledge, been adopted by any astronomical observer. No hypothesis of the possible origin of the sun's spots upon this idea has been framed that affords a satisfactory explanation of even their more conspicuous features and phenomena. To this remark the theory advanced by Professor Zöllner, viz. that "the nucleus of the solar spots is a scoriaceous product of local cooling on a liquid surface, and the penumbræ clouds of condensation which surround at a certain height the coasts of these islands of slag," offers no exception. It is not new, and has been already overthrown by the investigations of M. Faye[†]. The other fundamental hypothesis of Professor

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[*] Philosophical Magazine, Nov. 1870.
[†] *Comptes Rendus.* vol. lxi. p. 1089.

Zöllner's theory of the physical constitution of the sun, viz. that the masses which burst out into protuberances are local collections in bubble-like caverns which form in the superficial layers of a liquid glowing mass, does not derive any support from analogical facts. It must be regarded as a pure hypothesis, unsustained by any inherent probability, or by any known fact other than that which it is framed to explain. Besides, an hypothesis which brings the hydrogen in eruptive or streaming masses to the sun's surface does not suffice. Another arbitrary hypothesis is required to dispose of the hydrogen which has thus been accumulating above the sun's photosphere for an indefinite period of time.

Such being the state of the case with regard to the recent attempts to discover the secret of the sun's physical constitution, in the light of the late remarkable discoveries, we seem to be in this dilemma : whatever conception is formed of the condition of the sun's photosphere, whether liquid or gaseous, it appears to be contradicted by received principles, or controverted by established facts. We are thus naturally led to suspect that either some physical cause has been hitherto left out of account which plays an important part in solar phenomena, or else the conception adopted of the mechanical condition of the solar vapours is radically at fault. It appears to me that good and sufficient reasons may be urged that will justify both these grounds of suspicion, and that a new point of view may be gained from which we may obtain a deeper insight into the physical processes in operation on the sun.

It is a little remarkable that it should have been hitherto assumed that the facts and laws of terrestrial physics can alone furnish a true philosophical ground for a theory of solar physics, and that no serious attempt should have been made to obtain additional light from known processes in operation on a cosmical scale in the regions of space. There is a class of bodies, some of which in their periodical excursions through the fields of space approach quite near the sun, and which are in general conspicuously subject to influences of a powerful nature exerted by the sun, besides the force of gravitation. It certainly seems natural to expect that these cosmical bodies might give us some insight into the nature of the forces in operation at the sun's surface. It is assuredly too late to urge that the transformations which they undergo under the sun's influence are wholly involved in mystery ; for it has certainly been satisfactorily established that a portion of the cometary matter becomes subject to a *solar repulsion*, and is urged away by this force with a high velocity and to great distances from the sun, and that this repulsion augments in intensity as the comet ap-

proaches the sun*. Now, if such an energetic force of repulsion emanates from the sun and operates on cometic matter at all distances, both small and great, according to the law of inverse squares, there is assuredly a high probability that it may play an important part on that vast arena where solar forces are obviously engaged in fierce contention. It may be conjectured that the solar vapours are entirely different substances from, and wholly unlike in their physical state, the cometary vapours that appear to be so exceedingly subtle. But it is certainly more philosophical to suppose that the same substances, or substances possessed of the same general properties, are present in all cosmical bodies and the earth. Besides, we are not without direct evidence on this point. Huggins, by examining the spectrum furnished by the light emitted from the Comet II., 1868, detected the presence of the vapour of carbon in the brighter portions of the comet. " He has been able to discriminate between the light of the nucleus of a comet and that of its tail. The nucleus is self-luminous, and its substance is in the form of ignited gas. The coma shines by reflected light as clouds do."

If, as is now conceded by astronomers, the tail of a comet is made up of matter detached from the general mass of the comet by reason of a repulsive action exerted by the sun, it must also be admitted that the matter expelled is not all urged away by the same intensity of force and with the same velocity ; for we find that it is much more widely dispersed in the plane of the cometary orbit than is consistent with this supposition. For example, I have shown in my theoretical discussion of Donati's comet† that, if we conceive particles of matter to have been expelled from this comet with a certain small lateral velocity, and urged away during a certain interval of time by a solar repulsion bearing to the force of gravitation the ratio of 1.213 to 1, they would at the end of the interval have been found distributed over a narrow band coincident at its forward line with the curved preceding side of the tail of the comet, and that the other portions of the tail must have been composed of matter subject to various degrees of solar repulsion less than this. In fact, the definite conclusion was in this way reached that the preceding half of the tail consisted of matter repelled from the sun with a force varying between the limits 1.213 and 0, and

* This is generally, if not universally, admitted by astronomers. The author has undertaken in former Numbers of Silliman's Journal to establish by rigorous calculation that the luminous train of Donati's comet was developed by a force of solar repulsion cooperating with the attraction of gravitation, both varying according to the law of the inverse squares. (See Silliman's Journal, vol. xxxii.)

† Silliman's Journal, II. vol. xxxii. pp. 54-66.

that the following half was for the most part composed of matter detached from the comet simply by reason of a weakened gravitation toward the sun, the intensity of the force of gravitation along the following side of the tail being 0·455. There would seem then to be no alternative but to admit that the tail of Donati's comet was composed of different substances (or else of one substance in different physical states), subject to a repulsive action from the sun of various degrees of intensity, and either prevailing over the sun's attraction of gravitation or partially counteracting it, and so giving rise to an effective repulsion for certain of these substances and to a diminished gravitation for others. The simplest theoretical explanation that can be given of this state of things is to suppose that the *solar repulsion consists of a series of impulses propagated in waves through the æther of space and taking effect upon atoms of different sizes with varying intensity.* It is obvious that, if this be true, the smaller the atom the more effective should be the repulsion as compared with the gravitating force soliciting the atom, since the ratio of the two forces should be proportional to the surface divided by the volume of the atom, assuming that the mass is proportional to the volume, or that all atoms have the same density. We are thus incidentally led to infer that the larger comets consist of a variety of substances like the earth.

The question now arises, what can be the origin of the force of solar repulsion. There is another side of the diversified picture presented by cometary transformations under the sun's influence, which gives some intimations on this point. Not only is a certain portion of the cometary matter repelled by the sun, but it is also repelled by the nucleus of the comet. We see in large comets a series of envelopes rise at intervals from the nucleus on the side turned toward the sun, and recede at a nearly uniform rate until they become dissipated by the sun's repulsion. Luminous jets also stream out at times from the same side of the nucleus. These phenomena, it can hardly be doubted, are in some way the effect of the sun's heat. The simplest and most probable conclusion is, that the ejecting force which is brought into play by the sun's heat is the direct repulsive energy of the heat received by the comet. We are thus led to infer that *the repulsive action exerted by the sun upon matter in the state of the cometary vapours probably consists, either wholly or partially, in repulsive impulses propagated in the heat-waves proceeding from the sun.*

Let us now see whether any confirmation of these inferences, and any additional light in the direction of our present inquiry, can be obtained on the substantial ground of terrestrial physics. The definite question presented for consideration is whether the

results of observation or experiment afford any indication of a direct repulsive action exerted by radiant heat on the atoms of bodies. It is universally admitted that radiant heat, when imbibed by a body, acts as a repulsive or separating agency among its molecules. It is also conceived that the conduction of heat is by radiation from atom to atom. The most natural inference from these facts is that the waves of radiant heat which pass from atom to atom directly urge the atoms away from each other by repulsive impulses. Instead, however, of adopting this simple idea, physicists have generally been inclined to refer the expansion of bodies from heat to some mode of motion of the atoms originated by the heat received, though no detailed satisfactory explanation has yet been given of the manner in which such motions would directly originate an expansion. Strangely enough, this notion is even entertained by physicists who regard heat as the only cause of the repulsion subsisting among the molecules of bodies. It should here be noted that if the expansion of all bodies of matter from heat is to be ascribed to a direct impulsive or repulsive action of heat-waves proceeding from one atom and falling upon the surrounding atoms, then this force takes effect at the greatest distances by which the atoms are separated in the rarest gas under the feeblest pressure; and we should thus be led to expect that heated bodies in contact with each other might manifest signs of repulsion.

In point of fact many evidences of a heat-repulsion subsisting between particles of different bodies in contact or in close proximity have been adduced by different experimentalists and writers on physics, some of which may be briefly mentioned.

1. "When pure silica in an extreme state of division is highly heated, the slightest motion then causes the particles of the powder to slide over each other, and the surface of the powder is thrown into undulations almost like those of a liquid."

2. A rise of temperature is attended with a decrease of capillary attraction. Also the frictional resistance to the flow of water in pipes is diminished by heat.

3. "The spheroidal state of liquids is a complicated result of four distinct causes. The most influential is the repulsive force which heat exerts between objects which are closely approximated to each other "*.

4. The vibrations of heated metals, as shown in "Trevilyan's instrument" or "rocker," resting on a block of metal, are probably due to the direct repulsive force of heat, as maintained by Professor Forbes, of Edinburgh, in opposition to Faraday,

* Miller's ' Physics,' p. 285.

who conceived that they were attributable to the sudden expansion by heat of the body on which the rocker rests. Faraday's explanation of these curious phenomena is adopted by Tyndall, who endeavours to overthrow Professor Forbes's theory. His experiments serve to disclose the fallacy of certain features of the theory, but do not dislodge the fundamental idea that the phenomena are due to a force of heat-repulsion. This might be made apparent if we had space for a statement of the general principles on which the explanation rests, and for a detailed discussion of the results of the experiments.

Let us now consider if any sufficient evidence exists of a general force of molecular repulsion in operation at all temperatures, beyond the sphere of sensible adhesion or cohesion. We need, in fact, to look no further for this than to the simple fact that in the ordinary contact of bodies the interval of distance between them, minute as it is, much exceeds the range of the attraction of cohesion or adhesion; for in such contact the weight of the upper body is counteracted by a repulsion between the molecules about the point of contact. Dr. Robinson has shown, in his ' System of Mechanical Philosophy,' that if two glasses, one slightly convex the other flat, are placed on each other and pressed by a force of 1000 pounds to the square inch, they are still at the distance from each other of the thickness of the top of a soap-bubble just before it bursts, or at least $\frac{1}{4450}$ of an inch. In effecting this contact there was no evidence of any attraction existing at distances greater than that at which the contact occurred. A similar remark may be made with regard to all cases of the apparent contact of homogeneous substances under a moderate pressure. It is only by increasing the pressure more or less that the contiguous particles can be brought within the range of their reciprocal attraction of cohesion. When the particles are readily displaced among themselves under the direct action of a pressure or blow, as in the case of soft or malleable substances, a permanent union may be effected without difficulty between the surfaces; that is, the outer repulsion of some of the particles may be overcome, their attraction of cohesion brought into play at the reduced distance, and an equilibrium established at the neutral point between this attraction and the inner repulsion. This occurs in the welding of iron.

Other evidences of an effective repulsion in operation between the molecules of bodies in contact, or in close proximity, are cited in treatises on physics, although it is not always distinctly recognized that the sphere of its action lies entirely without that of the effective molecular attraction. Now, what is the range of this *effective* repulsion between bodies? It obviously extends

only to a small distance. Cavendish's well-known experiment has established that, when bodies are separated by considerable distances, they tend toward each other by the attraction of gravitation. But are we therefore to conclude that the repulsive action, so energetic at the near approach of the molecules, has vanished altogether when they are a considerable distance apart? Is it not more probable that this force is confined to the surface-molecules, and disappears at moderate distances, in comparison with the attraction of gravitation which is the result of the action of the entire masses on each other, because it decreases according to the inverse squares of the distance between the surfaces instead of the distance between the centres? I have elsewhere shown* that the force of gravitation cannot be the attraction of cohesion operating at considerable or great distances. It is a force *sui generis,* entirely distinct from the forces of molecular attraction and repulsion in operation at minute distances and determining the constitution of bodies and their mechanical properties, and operates in conjunction with, but independently of, these molecular forces†.

In view of the concurrent testimony that we have now seen is afforded by the two departments of cometary and terrestrial physics, it will be admitted that, in attempting to gain a new insight into the physical constitution of the sun and the processes of change in operation on its surface, we are at least entitled to assume the following as probable hypotheses :—

1. That the sun exercises a repulsive action upon the molecules of every gas or vapour that subsists at its surface, or is at any time in any part of the region of space exterior to the surface ; that this force is the sum of all the heat-impulses propagated in æthereal waves from all the gaseous molecules posited

* Philosophical Magazine, vol. xxxviii. p. 38.

† It ought here to be stated that in my paper " On Molecular Physics," published in Silliman's Journal, vols. xxxviii., xxxix.,and xl., I have deduced from the fundamental conception of a primitive molecule (or chemical atom) adopted a force of molecular repulsion operating beyond the sphere of the molecular or cohesive attraction, and reached the conclusion that this force has its immediate origin in the physical change to which the development of heat is in every instance due, viz. an inward or contractile vibratory movement of the electric envelopes by which all atoms are conceived to be surrounded as well as by æthereal atmospheres. It is accordingly termed the molecular heat-repulsion. The repulsive energy of heat in operation on any molecule is the sum of all the æthereal impulses developed by such movements of the envelopes of other molecules (whether originating in the attraction exerted by the central atoms on their envelopes, or in an external collision or pressure), and propagated to the molecule. From the principle of interception of wave-force it results that the external repulsive action exerted by a solid body is confined to the surface-molecules; while a force of heat-repulsion is propagated to an indefinite distance from all the molecules of a gas.

above the solid or liquid body of the sun, and from the surface-molecules of this central mass, except in so far as these impulses may be intercepted in their passage; and that it is opposed to the force of gravitation, which is due to a virtual attraction of the sun's entire mass, so that the effective force soliciting any gaseous molecule is the difference between these two forces (attractive and repulsive) by which it is urged.

2. That the force of solar repulsion, since it consists of impulses propagated in æthereal waves, is comparatively more effective in proportion as the atomic weight of the solar vapour is less, it being assumed that the quantity of matter in any atom is proportional to its volume.

3. That in a hypothetical condition of equilibrium of the sun's atmosphere the elastic force of each of its vaporous constituents at any depth will consist in the intensity of the effective heat-impulses tending to urge its molecules outward, which will be counteracted by the weight of the superincumbent portion of the atmosphere. Now let us assume, for the moment, that at some anterior epoch in the sun's history all the present vaporous constituents of the sun's atmosphere were diffused throughout a space exterior to the central body of the sun, and limited by the spherical surface (A) at which the molecules of the vapour of greatest atomic weight are in equilibrium under the action of their own weight and of the heat-repulsion urging them upward. This hypothetical state of things could not continue, since the atoms of each of the other solar vapours would be urged upward by an effective force. If we conceive a small quantity of each of them to escape from all points of this surface, the rising vapours will ascend to greater heights in proportion as their atomic weights are less, and finally, when the equilibrium is attained, form a series of spherical envelopes wholly detached from each other, and arranged in the order of atomic weights—beginning with the heavier metallic vapours and terminating with the lighter (potassium, sodium, &c.) and the permanent gases, with hydrogen outermost. If other small portions of each of the vapours were to rise from the surface A, they would serve to augment the thickness of the envelopes already formed; and the same would be true for each successive discharge. The final result would be the same if the discharge were continuous during a certain interval of time, as would naturally happen. After a certain amount of the solar vapours have escaped, contiguous envelopes might interpenetrate each other more or less. What it is especially important to observe is, that *throughout the whole depth intercepted between each envelope and the outer limiting surface* A *of the vapour of greatest atomic weight, every atom of the substance of which the*

envelope is composed that may chance to be present is urged up-
ward by a force of repulsion. If, as we must suppose, the rise
of the solar vapours from the surface A continued for an indefi-
nite time, the interpenetration of contiguous envelopes would
increase, and eventually a condition of equilibrium would be
attained, if the sun's temperature remained the same. But if
this temperature were to increase, as it must down to a certain
epoch in the process of consolidation, the process above indi-
cated would be continually renewed. It is still more important
to observe that if there were any cause in operation withdraw-
ing continually at short intervals a portion of one or more of
these rising vapours, a statical equilibrium would not be reached ;
and it would be permanently true that *for every such vapour there
would be a region of repulsion,* as above stated, *extending from its
envelope down to the outer limit* A *of the vapour of greatest atomic
weight.* Throughout this region the vapour would be perpetu-
ally rising, taking the place of that which is withdrawn, and so
maintaining a *dynamical equilibrium.* The depth of this region
would be the greatest for hydrogen, the outermost gas (unless
there is some solar vapour of less atomic weight than hydrogen).
Now it is easy to see that a certain physical cause tending to
produce such results must come into operation at some stage
of the sun's process of consolidation. As conceived by Faye,
the cooling going on at the outer surface must eventually bring
the temperature there down to the point at which the vapours
having the highest affinity for oxygen will undergo combus-
tion. The product of such combustion, being compound mole-
cules, will have a greater weight in comparison with the repul-
sion to which they are exposed than the simple molecules
before the combination took place, and hence they will descend
more or less rapidly into the depths of the photosphere. To
all appearance the sun is now passing through this period of its
physical history, as supposed by Faye ; and in the "granula-
tions" which give to the solar disk a mottled appearance
(Herschel's "subsiding chemical precipitates") we probably
discern the products of the combustion occurring in the upper
photosphere and determining its outer limit in the act of de-
scending. The continual upward flow, from the depths of the
photosphere, of the hydrogen, oxygen, and the lighter metallic
vapours will bring about the necessary intermixture of oxygen
with the other vapours. This must occur below the natural
outer limit of the hydrogen envelope ; and we know that, as a
matter of fact, the chromosphere, composed chiefly of hydrogen,
extends above the photosphere.

If the products of the surface-combustion were all to descend
indefinitely into the vaporous photosphere without undergoing

decomposition, no further visible effects would ensue. We could only follow with the mind's eye the gradual growth of the central nucleus of the sun, and recognize that we probably have before us a picture of the process by which the materials of the earth's crust were fashioned and accumulated in the earlier ages of its history. But the probability is that the descending masses would eventually arrive at a depth where the higher temperature would effect a dissociation of the combined elements (as Faye supposes). This must inevitably happen unless the tendency of the heat that augments with the depth is counteracted by the opposing tendency of the increasing gaseous pressure. By reason of these opposing tendencies it may well happen that there may be a certain region of · dissociation of limited depth, above and below which decomposition would not occur. But it is to be observed that it does not follow that all of the products of surface-combustion as they pass through such a region would be decomposed, since the reduction of temperature attending every instance of decomposition tends to prevent decomposition of other surrounding masses in the act of descending.

This sudden dissociation of large masses of combined elements, though occurring at certain depths within the photosphere, it will be seen, may eventually play a conspicuous part at the surface. Unless the region of dissociation should lie below that of repulsion for the elements separated, these elements after separation will be urged upward by the effective force of repulsion, ascend rapidly, and emerge with a high velocity above their respective envelopes. The ascensional velocities attained will be greater if large masses are suddenly decomposed. The masses of hydrogen set free should attain to the greatest velocity and rise to the greatest height. They should rise in eruptive masses above the hydrogen envelope, or, in other words, the chromosphere. According to Lockyer, in the solar protuberances the ascending hydrogen has in some cases a velocity as high as 120 miles per second, and rises to a height of more than 40,000 miles.

A vertical jet of hydrogen having a projectile velocity of 120 miles per second should attain an altitude of 43,000 miles, if the solar gravity were constant for that altitude. Some prominences have extended to a height of 100,000 miles above the sun's photosphere. Professor Respighi has even noticed instances of an elevation of 160,000 miles. Such enormous heights imply either a greater initial velocity than 120 miles per second, or that the full energy of the solar attraction does not take effect on the eruptive masses of hydrogen in the region above the photosphere. From our theoretical point of view we

perceive that the latter supposition should be true, since the solar repulsion should be in operation above the hydrogen envelope or the chromosphere, diminishing the gravitating tendency.

The metallic vapours set free in the region of dissociation should rise to heights varying with their atomic weights. Some of them, especially the lighter ones (sodium, magnesium, calcium, &c.), may acquire velocities sufficient to bring them above the chromosphere. In fact, the spectroscope has detected, besides hydrogen, magnesium, sodium, iron, and chromium in the solar protuberances. Lockyer states that he has invariably found that in solar storms the chromospheric layers are thrown up in the order of vapour-density. He regards the chromosphere as built up of the following layers, which are in the order of vapour-density in the case of known elements :—a new element giving the green coronal line in the spectroscope, hydrogen, another new element, magnesium, sodium, barium, iron. He remarks that " all the heavier vapours are at or below the level of the photosphere itself "*.

The green coronal line was traced in the late eclipse by Professors Young and Winlock as far as 16′, or 425,000 miles from the sun's limb. From our present theoretical stand-point we naturally infer, as Lockyer has already done from his observations, that the element present in the solar corona which gives this line is much lighter than hydrogen. We see also that an element several times lighter than hydrogen might be subject to a solar repulsion that would predominate over the attraction of gravitation at all distances, and urge the subtle vapour indefinitely away from the sun. Since the same line is seen in the light of terrestrial auroras, we must conclude that the same substance is present in our upper atmosphere, either in a permanent upper layer or derived from the sun (as I have elsewhere maintained). We must infer also that it is magnetic, which apparently cannot be the case unless it takes on the condition of compound molecules. Such compound molecules might become dispersed in the upper atmosphere of the earth, or in the photosphere of the sun, by electric discharges or sudden evolutions of heat, and then the separate atoms repelled off, forming the streamers of the corona and aurora, illuminated either by electric light or, in the case of the corona, in part also by reflected solar light †.

* It is admitted by Ångström and Zöllner that the absence of spectroscopic indications of oxygen and nitrogen in the sun is no sufficient evidence that these gases are really wanting in the sun's atmosphere.

† It is also conceivable that the subtle vapour streaming off in the coronal rays has been set free by dissociation, like the hydrogen, from some

The probable origin of the sun's spots and other questions of solar physics that claim attention must be left for future consideration. I will only remark here that it has long been apparent that the diverse phenomena which occur at the sun's surface are traceable, more or less directly, to the action of some form of eruptive force. *The present investigation seems to have led to the discovery of the true nature and origin of this force, and at the same time to have revealed the process by which the sun's radiation is maintained,* the primary source of the solar heat being doubtless, as now generally believed, the process of condensation maintained by the force of gravitation.

It is worthy of remark, in conclusion, that as comets directed our attention at the outset toward the sun, so the sun, in its turn, leads us back again to our starting-point, since we see that if we transfer to cometary bodies the physical structure we have recognized in the sun's upper photosphere, viz. the existence of a succession of light vaporous envelopes subject to the energetic action of the force of heat-repulsion, the mystery in which some of the curious transformations they undergo have hitherto been involved seems to be in a great degree dispelled. No one doubts that comets are chiefly composed of very light vapours, though some of the larger ones may have a solid nucleus. If, as intimated, certain observed cometary phenomena indicate that these vapours, like the solar vapours, are arranged, for a certain depth at least, in envelopes which are liable to be greatly expanded, or even wholly expelled by the increasing amount of heat received from the sun, we have in the probable physical structure of comets another indication that these bodies were originally detached from the sun's photosphere, in addition to that furnished by certain features of the cometary motions*.

VII. *On Increasing the Rigidity of long, thin Metallic Pointers, Magnetic Needles, &c. By* JOHN C. DOUGLAS, *Science Teacher, G.B., E.I. Gov. Telegraph Department*†.

THE needles and needle-indices of galvanometers, when long and (to ensure lightness) thin, are very liable to become bent; or to prevent this bending and admit of the pointer being brought

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other element of a compound substance in the depths of the photosphere. However this may be, it can hardly be doubted that the ascent and descent of the solar vapours and the combinations and decompositions going on among them must be attended with disturbances of electric equilibrium, from which decided effects must result.

  * Norton's 'Astronomy,' revised edition, p. 276.

  † Communicated by the Author.

close to the scale, the weight of the suspended mass is frequently increased at the expense of delicacy.

Where it is required to obtain an index or needle long, light, and at the same time rigid, I suggest that the thin metal be buckled: an ordinary steel pen is an example of a thin piece of metal rendered very rigid by buckling in the manner proposed; but the increased rigidity bestowed by such alteration of form is so well understood as to render it unnecessary to dwell on it here. An index would, of course, be bent in the direction of its length, and the shape of the cross section might be varied.

It is evident that with a given weight a longer, and with a given length a lighter index is possible if the metal be buckled instead of used plane as at present; a longer and finer point may be bestowed on the buckled than on the unbuckled index; and the greater rigidity of the buckled index would render it possible to approach it nearer to the scale of the instrument without fear of contact.

I have alluded to galvanometers particularly; but the improvement suggested is applicable to any instrument in which a long and sharp yet light index is a desideratum, while it may not be required in some cases, where weight cannot be reduced by reason of the necessity for a certain magnetic mass.

I have stated the principle as applicable to indices composed of metal, but it is applicable to other materials, as horn, vulcanite, &c.; aluminium and brass, however, are probably most generally applicable and would usually be employed. Slight modifications in the mode of attachment might be rendered necessary by the buckling of the index; but such details cannot present any difficulty, while the improvement would be appreciated by all in the habit of using instruments with long, light indices as at present constructed. I have seen clock-hands in which the principle has been partially applied; but in such cases it is scarcely necessary, and ornament appears to be generally the object. The subject of this paper suggested itself to me from the difficulty I have found in keeping the needles of detectors &c. flat in spite of the handling of them in remagnetizing, and in using delicate galvanometers, the long light needles and long thin brass pointers of which have to be occasionally handled, are very readily bent, and by no means readily rendered straight again.

VIII. *Notices respecting New Books.*

*Astronomy Simplified for General Reading, with numerous New Ex-
planations and Discoveries in Spectrum Analysis, &c.* By J. A.
S. ROLLWYN. Tegg, London.

POSSESSING a knowledge of astronomical literature dating from
1811, when we first made acquaintance with Ferguson's ' Young
Ladies' and Gentlemen's Astronomy,' now scarce, and, as time pro-
gressed, with Brewster, Woodhouse, Herschel, Vince, Drew, and a
host of other writers, including the memoirs which appeared from
time to time in the ' Transactions ' of scientific bodies, &c., we anti-
cipated, from the title of the work before us, a clear and lucid state-
ment of the broad and leading features of astronomy, couched in
language which would render them easily intelligible to readers
who were not desirous of mastering the more abstruse portions of
the science.   Upon reviewing our perusal of 402 pages we look back
on our literary journey as having passed through regions of black and
white, in which we have encountered illusory nebulæ, pear-shaped
moons, smoking suns, hypothetical planets, atmospheric star-showers,
suggestions for a sensational novel describing life in the planet Mars,
allusions to wax-doll astronomy, a chemical theory of Saturn's rings,
and a new explanation of the dark lines in the solar spectrum ; and
we seriously inquire, Have we mistaken the title ? is it really
Astronomy Simplified ?   For most assuredly we should have arrived
at the conclusion that " Astronomy Mystified " would have been
the better title, had not the title-page itself informed us of the sim-
plicity of the work and its suitability for general reading.

Amongst the numerous books, popular and otherwise, that have
been written on astronomy, there is much that may be characterized
as calculated to furnish general and accurate ideas of stars and star-
systems, of nebulæ, of the sun and his planetary family, of comets,
and of the connexion between astronomy and chemistry through the
medium of spectrum analysis ; and had the author abstained from
controverting theories which are likely to be established, and intro-
ducing those of his own which have to pass through the necessary
ordeal, he would, we are convinced from various passages in the
work, have succeeded in producing a book which would have an-
swered to its title.   For example, had he grouped selections of *known
facts* under heads as above mentioned, referring but sparingly to
theories, and to those only which would clearly explain classes of
facts, his work would have taken rank with the productions of the
popular scientific writers of the day.   But he has gone out of his way
to guard the public against what he terms the authority of science ;
he says in his preface, " that while many things go forth to the
general world apparently under the authority of science, which are
only, after all, matter of speculation, without any scientific sanction
* * * *   The public mind is, as a rule, not sufficiently acquainted
with the rationalia involved to be capable of self-protection, or to
be able to distinguish, as scientific men generally do, what has been
definitely discovered from what has in the absence of discovery been

plausibly suggested." In the outset Mr. Rollwyn stands forth as the champion of the non-scientific, to defend them against the plausible suggestions of time-honoured philosophers, who have given to the world, by means of observation and theory, the facts and explanations of the constituent bodies of the universe, substituting his new explanations and, as he calls them, discoveries, for much that is now generally received.

In that portion of the work which is devoted to the consideration of stars, star-systems, and nebulæ, it would have contributed greatly to simplicity had the author, in addition to the array of figures expressing *hundreds of billions*, given the quantities in so many words. The experience of the general public, for whom Mr. Rollwyn writes, in estimating quantities seldom exceeds *hundreds of thousands*, and as a billion is a million times a million, it is by a great mental effort that so large a quantity can be apprehended by figures alone. With regard to stellar arrangements, Mr. Rollwyn does not appear to be well read up ; for in his work we look in vain for any notice of Proctor's theories of star-drift, star-streams, the constitution of the universe, &c. ; and it is remarkable that, amongst theories which have been regarded with but little favour by the author, the speculations of Proctor should have been overlooked. On the other hand we find the author demolishing, as he thinks, the gaseous character of the nebulæ, calling in question the results of Dr. Huggins's observations of the bright lines in the spectra of nebulæ, as well as the observations of Sir W. Herschel, whose telescope, the author says, invests and unites the Dumb Bell with a fictitious elliptical halo and junction. " How can such luminosity yield a spectrum analysis ? We should as soon think of testing the spectrum analysis of a ghost, or a night-mare, or a dream. To come to the conclusion that because such a spectrum presents no elementary lines it must be the spectrum of a vapour, would be vapouring indeed, and with very thin vapour. All honour to spectrum analysis ! but we trust we are too jealous of the integrity of scientific truth to accept implicitly the first suggestions and unfledged conclusions drawn from it,—too appreciative of its great achievements, too respectful to its already well-won and wonderful laurels, to drag it through the mire in support of hasty and ill-digested inferences."

Of the three bright lines in the spectra of nebulæ discovered by Dr. Huggins, Mr. Rollwyn remarks, "it is surely exacting too much to ask us to assume that when three lines only appear in the spectroscope, this miserable meagre telegram exhaustively explains all the luminosity embraced within the extended confine."

It would be wasting time to go further into an analysis of the work. Not only is the author ill-read in his subject and extravagant in his suppositions (of which the pear-shaped form of the moon is an example, the diameter directed towards the earth being 3893 miles, while that at right angles to it is 2153 : whence he obtained his data we are at a loss to conceive ; the micrometrical measurements of Gussew in 1859 or 1860 gave the greatest mean deviation from the spherical form as about $\frac{1}{20}$ of the radius, or less than 60 miles),

but his misquotations and errors are numerous, so that as a work of reference it is valueless. The Astronomer Royal's discovery of the inequality of the motions of the Earth and Venus, the period of which is 240 years, is ascribed to Le Verrier, with not one word of the laborious work of the Astronomer Royal. The work concludes with the announcement that it is demonstrable that the area of the circle is equal to three fourths of the square of its diameter.

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xli. p. 546.]

March 9, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communication was read :—
  "Results of Seven Years' Observations of the Dip and Horizontal Force at Stonyhurst College Observatory, from April 1863 to March 1870." By the Rev. S. J. Perry.

The object of the present paper is to bring further evidence to bear upon an important question of terrestrial magnetism.

The existence of a sensible semiannual inequality in the earth's magnetic elements, dependent on the position of the sun in the ecliptic, was deduced by General Sir Edward Sabine from a discussion in 1863 of a continuous series of the monthly magnetic observations taken at Kew. A previous reduction of observations made at Hobarton and at Toronto had first suggested the idea, and a new confirmation of the results has lately been obtained by Dr. Balfour Stewart from subjecting a second series of Kew observations to the same tests as before. The observations, which form the basis of the present discussion, extend over the period from March 1863 to February 1870, during which time the same instruments have been in constant use. These are a Jones unifilar and a dip-circle by Barrow, both tested at Kew, and a Frodsham chronometer. Sir Edward Sabine, who made the Stonyhurst Observatory one of his magnetic stations in the English survey in 1858, greatly encouraged the undertaking of monthly magnetic observations, and the Rev. A. Weld procured in consequence the instruments still in use. Only occasional observations were made with these instruments for some years, and it was only in 1863 that a continuous series of monthly determinations of the magnetic elements was started by the Rev. W. Sidgreaves. He observed regularly until September 1868, when I returned to my former post at the Observatory, and I have continued the same work ever since.

A stone pillar was at first erected for the magnetic instruments in the open garden, and this remained in use from 1858 until the beginning of 1868, when a most convenient hut of glass and wood was built for the instruments in a retired corner of the College garden. This alteration was rendered necessary from the placing of iron rails in the vicinity of the old pillar ; and although it introduces into the

results a correction for change of station, it has the great advantage of securing immunity from disturbance for the future.

Considering the object in view in drawing up this reduced form of the dip and horizontal-force observations, I have judged it ad-visable to adhere strictly to the tabular forms in which the matter has been presented in previous discussions of a similar nature. Each element is the subject matter of these tables. In the first are the monthly values of the element, the deduced mean value, and its se-cular variation. Next in order comes the calculation of the semi-annual inequality. The residual errors, and consequent probable weights of the observations and results, compose the third and last Table.

The yearly mean values of the horizontal force are found to vary progressively from 3·5926 to 3·6178 in British units, the mean for Oct. 1st, 1866, being 3·6034, with a secular acceleration of 0·0042. Calculating from the monthly Tables the mean value of the horizontal force for the six months from April to September, and for the semi-annual period from October to March, we find the former to be 0·0005 in excess over the latter, showing that this component of the intensity is greater during the summer than during the winter months. Treating the dip observations in a precisely similar way, we obtain 69° 45′ 21″ as the mean value of this element for October 1st, 1866, subject to a secular diminution of 1′ 49″·2; the extreme yearly means being 69° 48′ 47″ and 69° 37′ 52″. The resulting ex-cess of 10″ for the winter months in the computed semiannual means is so small, that the observations tend mainly to show that the effect of the sun's position is not clearly manifested by any de-cided variation in the dip. Deducing the intensity from the above elements, we obtain for the summer months the value 10·4136, whilst that for the winter months is 10·4128. The intensity of the earth's magnetic force would thus appear to increase with the sun's distance, but the difference is not large enough to have more than a negative weight in the question under discussion. This weight, moreover, is lessened by the slight uncertainty arising from the probable disturbing causes at the first magnetic station.

It is hoped that a second series of observations at the new station will throw greater light on the fact of the sun's influence on terres-trial magnetism, by either confirming the results obtained above, or by adding fresh weight to the conclusions arrived at by the President of the Royal Society.

March 23.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communications were read :—

" On an approximately Decennial Variation of the Temperature at the Observatory at the Cape of Good Hope between the years 1841 and 1870, viewed in connexion with the Variation of the Solar Spots." By E. J. Stone, F.R.S., Astronomer Royal at the Cape of Good Hope. In a Letter to the President.

Royal Observatory, Cape of Good Hope, Jan. 17, 1871.

Dear Sir,—I enclose a curve of the variation of the annual mean

temperature at the Cape deduced from observations extending from 1841 to 1870 inclusive. I have carefully examined the zero-points of all the thermometers which have been employed in this series of observations. I have then deduced the rate of change of these thermometers, from a comparison of the index-errors thus found and those given originally or obtained in 1852 by Sir Thomas Maclear, when he compared the principal thermometers at the Observatory with the readings of a standard " Regnault" which had been sent out to the Observatory for that purpose by you. These indications of change have been carefully checked by all the comparisons made, at different times and for different purposes, of these thermometers *inter se* and with others which still remain at the Observatory. From the agreement of the different results thus checked, I have no doubt upon my own mind of the systematic character and sensible amount of the increase of readings of thermometers with age thus indicated. In some cases the change appears to amount to as much as $0°\cdot05$ F. per annum. From these results I have deduced the index-errors of the different thermometers for the different periods, and applied these corrections throughout. I have also corrected the mean results of the five observations made daily since 1847 in order to deduce the true daily mean.

The results thus reduced on a general system, and extending over thirty years, appeared likely to afford information respecting any connexion which might exist between the mean temperature and the frequency of solar spots. I have therefore constructed the curves of variation of mean annual temperature, and the inverse curve of solar-spot frequency for comparison. The latter curve has been founded upon Wolf's observations.

The observations of temperature from 1841 to 1851 inclusive were made in the original Meteorological Observatory, which was burnt down in 1852, March 11.

The observations from 1852, April 24, to 1858, August 31, were made in a wooden shed erected for the purpose on the site of the old Observatory.

The observations from 1858, August 31st, to the present time have been made in the crib before the south-west window of the Transit-Circle Room.

These changes are so far unfortunate, that there is clearly a change of mean temperature arising from the different circumstances of exposure. I have therefore referred each set of observations to the mean temperature deduced from all the observations made under the same circumstances of exposure. The deviations of the mean temperature for each year from the mean of the whole period of similar exposure are then laid down as ordinates on the scale of one division of the ruled paper to $0°\cdot05$ F. To smooth down the irregularities, I have joined the points thus laid down, and bisected the lines thus joining these points whenever the corresponding mean temperatures were deduced from a full year's observations. In other cases the temperatures corresponding to the deficient months have been supplied from the adjoining years, and the resulting mean temperature allowed

less weight. The inverse curve of the frequency of solar spots has
been formed by simply subtracting 100 from Wolf's numbers, and
laying down points to the scale of a number 4 to 0°·05 F., or one
division of the ruled paper.

The broken curve represents the variations in the mean annual temperature
at the Cape ; the continuous line is the inverse curve of solar spots' frequency.

The agreement between the curves appears to me so close that I
cannot but believe that the same cause which leads to an excess of
mean annual temperature leads equally to a dissipation of solar spots.
There is on the whole a curious appearance of lagging of the inverse

curve of solar spots over that of temperature. At the maximum about 1856, however, this does not appear to be the case ; but when the uncertainties of the data, both of the solar spots near the mini-mum, and of the mean temperature also, are taken into account, such discrepancies might perhaps fairly be expected, even if there be a physical connexion between the two phenomena as results of some common cause. If there be a sensible inequality in the mean temperature with a period of about ten years, then the mean tem-perature resulting from the observations in the temporary Observa-tory, which were made near a maximum, will be too high. The corresponding ordinates, therefore, will be depressed too much rela-tively to those corresponding to observations made in the other two observatories. In the curve 2, I have imperfectly corrected the mean of the results for the temporary observatory on the supposition of such an inequality existing. The only result of such a correction is to modify the curve at the points of junction of the observations made in different positions. The general form is unaltered. It should be mentioned that the point about which the curves appear to differ most is near or at the change of exposure from the original observa-tory to the temporary shed, about 1852.

I may mention that I had not the slightest expectation, on first laying down the curves, of any sensible agreement resulting, but that I now consider the agreement too close to be a matter of chance. I should, however, rather lean to the opinion that the connexion be-tween the variation of mean temperature and the appearance of solar spots is indirect rather than direct, that each results from some ge-neral change of solar energy.

I have forwarded these curves to you, knowing the great interest you have ever taken in such inquiries, and on account of your being the chief promoter of the establishment of a Meteorological Observa-tory here. The problems of meteorology appear to be presented here in a simpler form than in England ; and probably systematic photo-graphic self-registering observations extended over a few years might lead to important results.

I have the honour to be, Sir,

Yours obediently,

E. J. Stone.

Sir Edward Sabine, K.C.B., P.R.S., &c.

Résumé of two Papers on Sun-spots :—" On the Form of the Sun-spot Curve," by Prof. Wolf ; and " On the Connexion of Sun-spots with Planetary Configuration," by M. Fritz. By B. Loewy.

Of these two series of investigations, one is by Professor Wolf, the other by M. Fritz, communicated to Prof. Wolf.

In the first, Prof. Wolf has proposed to himself to find the mean character of the curve of sun-spots, *i. e.* its real form from one minimum to another. He investigates the form only for $2\frac{1}{2}$ years before, and $2\frac{1}{2}$ years after each minimum, and concludes by a simple proportion of the remainder. He finds that the curve *ascends* more rapidly than it *descends*—the ascent taking in the mean 3·7 years,

the descent lasting 7·4 years. We have established these data far more reliably in our last paper; and our curve gives 3·52 years for the ascent, 7·54 years for the descent (average of the three periods). Professor Wolf also thinks that although a single period may differ essentially in its character and form from the mean, still, on the whole, if the descent is retarded, the ascent in the same period is also retarded; if the former is accelerated, the latter is also accelerated. This is not quite borne out by our curve. He also overlooks the secondary maximum, which may lead to great conclusions if more investigated together with other matters.

M. Fritz comes to the following conclusions :—

1. The connexion between sun-spots and auroral and magnetic disturbances indicates an external cause, to be sought in planetary configurations.

2. The relative influence of the planets must be exerted in the following order :—Jupiter (greatest), Venus, Mercury, Earth, Saturn.

3. This influence cannot entirely depend on the time of rotation ; but changes in the *magnetic* axes of these planets may have the most determining effect.

4. Investigating the comparative influences of them singly and together (as far as possible), at the times of conjunction and quadrature, he finds the greatest coincidence of maxima of sun-spots with the time when *Jupiter* and *Saturn* are in quadrature ; and the greatest coincidence of minima when these planets are in conjunction.

5. There is also (a minor) coincidence of maxima when Jupiter and Venus are in quadrature.

There is also an extension of the paper for finding the connexions with auroras, and a statement that every 27·7 days there seems to be a monthly maximum, which may probably be explained (according to Fritz) by the tendency of a particular solar meridian to spot-formation, depending upon the presence of an intra-Mercurial planet.

GEOLOGICAL SOCIETY.

[Continued from vol. xli. p. 549.]

January 25, 1871.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

The following communications were read :—

1. " On the Physical Relations of the New Red Marl, Rhætic beds, and Lower Lias." By Prof. A. C. Ramsay, LL.D., F.R.S., F.G.S.

The author commenced by stating that there is a perfect physical gradation between the New Red Marl and the Rhætic beds. He considered that the New Red Sandstone and Marl were formed in inland waters, the latter in a salt lake, and regarded the abundance of oxide of iron in them as favourable to this view. The fossil footprints occurring in them were evidence that there was no tide in the water. The author maintained that the New Red Marl is more closely related to the Rhætic, and even to the Lias, than to the

Bunter; and in support of this opinion he cited both stratigraphical and palæontological evidence. He described what he regarded as the sequence of events during the accumulation of the later Triassic deposits and the passage through the Rhætic to the Lias, and intimated that the same reasoning would apply to other British strata, especially some of those coloured red by oxide of iron, including the Permian, the Old Red Sandstone, and a part of the Cambrian.

2. " Note on a large Reptilian Skull from Brooke, Isle of Wight, probably Dinosaurian, and referable to the genus *Iguanodon*." By J. W. Hulke, Esq., F.R.S., F.G.S.

The author stated that the skull described by him was obtained from a Wealden deposit at Brooke, in the Isle of Wight, from which many remains of Dinosauria have been obtained. He described its characters in detail, and remarked that its most striking peculiarities were:—the completeness of the bony brain-case; the obliteration of the sutures, especially those of the basicranial axis; the massiveness of the skull; and the great downward extension of the basisphenoid, with the attendant upward slant of the lower border of the basipresphenoidal rod. The first of these characters occurs elsewhere among reptiles only in *Dicynodon*; and the first and second characters combined were regarded by the author as approximating the skull to the ornithic type. The reference of this skull to *Iguanodon* was founded chiefly on the place from which it was obtained, which has furnished abundant remains of that genus, and on the obliteration of the sutures, which the author stated to be a character of the mandibles of *Iguanodon*.

February 8, 1871.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. " On the Punfield Formation." By John W. Judd, Esq., F.G.S., of the Geological Survey of England and Wales.

Those formations which have been deposited under *fluvio-marine* conditions, and which yield at the same time marine, freshwater, and terrestrial fossils, are of especial interest to the geologist, as they furnish him with a means of correlating the great freshwater systems of strata with those of marine origin.

At the bottom of the Wealden we have one such fluvio-marine series, the well-known Purbeck formation; at its summit is another, less known, but not less important, for which the name of " Punfield Formation" is now suggested. Some of the fossils of the latter were first brought under the notice of geologists by Mr. Godwin-Austen in 1850; and their peculiarities have since been the subject of remark by Prof. E. Forbes, Sir C. Lyell, and others.

The typical section of the beds is at Punfield Cove, in the Isle of Purbeck, where they are about 160 feet thick, and include several bands with marine shells. The lowest and most remarkable of these yields about forty well-defined species, many of which, as well as

one of the genera, are quite new to this country. A section some-
what similar to that of Punfield, is seen at Worborrow Bay.

In the Isle of Wight, at Compton, Brixton, and Sandown Bays,
similar fluvio-marine beds are found at the top of the Wealden, and
attain to a thickness of 230 feet. The marine bands here, however,
yield but a very scanty fauna. Indications of the existence of beds
of the same character and in a similar position are found in the dis-
trict of the Weald.

While the Purbeck formation exhibits the gradual passage of the
marine Portlandian into the freshwater Wealden, the Punfield
formation shows the transition of the latter into the marine Upper
Neocomian (Lower Greensand). Thus we are led to conclude that
the epoch of the English Wealden commenced before the close of the
Jurassic period, lasted through the whole of the Tithonian and of
the Lower and Middle Neocomian, and only came to a close at the
commencement of the Upper Neocomian.

In tracing the Cretaceous strata proper from east to west, they
are found to undergo great modification ; while the Neocomian and
Wealden, which they overlap through unconformity, besides being
greatly changed in character, thin out very rapidly.

On stratigraphical and palæontological evidence, the Punfield
formation is clearly referable to the upper part of the Middle Neo-
comian. Its fauna has remarkably close analogies with that of the
great coal-bearing formation of eastern Spain, which is of vast
thickness and great economic value.

The claim of the Punfield beds, equally with the similarly situated
Purbeck series, to rank as a distinct formation, is founded on the
distinctness of their mineralogical characters, their great thickness,
the fact of their yielding a considerable and very well characterized
fauna, and of their being the equivalent of a highly important
foreign series.

2. " Some remarks on the Denudation of the Oolites of the Bath
district, with a theory on the Denudation of Oolites generally." By
W. Stephen Mitchell, Esq., M.A., F.G.S., of Gonville and Caius
College, Cambridge.

The author briefly referred to the theory according to which
oolitic deposits were supposed to have been originally spread out in
continuous sheets over the country which they occupy, and to owe
their division into separate hills to the action of denudation after
their original deposition and consolidation. He suggested, as an
equally probable hypothesis, that, whilst the marls and clays of
oolitic areas were probably originally deposited in continuous beds,
the limestones in many cases may never have extended beyond
the areas now occupied by them. He described the beds of lime-
stone in the oolitic hills as thinning out towards the valleys on all
sides, maintained that the limestones owed their origin to coral
reefs, and cited several descriptions of coral islands by Prof. Jukes,
to show the agreement in their structure with that which he ascribed

to the oolitic hills. He assumed that in the event of a coral-area becoming one of sedimentary deposition, the sedimentary deposit would preserve intact the contour of the coral islands, and inferred that this has been the case in the Bath district, so that the Great-Oolite cappings of the hills of that area may represent the original contours of coral islands, exposed by the denudation of the Bradford clay. The amount of denudation undergone by the Great-Ooliteli mestone he considered to be very small. The Inferior Oolite, on the contrary, he believed to have suffered denudation ; and he considered that the course of the valleys formed by this agent was dependent on the form of the limestones capping the hills.

X. *Intelligence and Miscellaneous Articles.*

ON THE MICROSCOPIC STRUCTURE OF HAIL.

BY PAUL REINSCH.

ALTHOUGH the formation and origin of hailstones must be counted among the meteorological phenomena which have not been fully explained, yet its differences from other atmospheric deposits (as, for instance, in structure) show that in its formation causes are at work which in the formation of other atmospheric deposits are either entirely wanting or only operate in less degree. Yet from the difference in hailstones as regards form, magnitude, and internal structure it must be concluded that the same causes are not always at work. In any case, in order to give a theory which shall completely explain the phenomena, it is important to know all types of hailstones ; more especially does the microscopic structure offer many criteria for a correct theory. In this subject of meteorology, as in many other branches of science, the microscope has a future before it. The present notice has reference to the microscopic structure of the hailstones of a storm which passed over part of the Westrich in the afternoon of June 8, 1869, a few days before the fall of the Krakenberg meteorite. The hailstones had a diameter of 10 to 12 millims., were almost exactly spherical, and appeared to have rather a concentric than a radial structure. An individual stone laid upon the object-table of a microscope was seen to be made up of individual corns or granules all of nearly the same size, in the middle of each of which was a single minute bubble with a brighter core and darker periphery. The individual granules are ordinarily round, but sometimes elongated ; they are bounded by a well-defined line, and are sharply separated from each other ; so that part of a hailstone shows some similarity to the merenchymatic structure of the vegetable cell ; the substance of the grain itself is quite homogeneous and free from structure*. As the hailstone gradually melts on the surface of the object-table the following remarkable deportment is observed. The darker sharp contour of the granule disappears at the fusion-line

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* I was unfortunately unable to observe the structure of the solid substance of the grain in polarized light.

of the hailstone, without distinct indications of a different deportment from the rest of the solid apparently homogeneous mass of the granule. But as soon as the line of fusion reaches the spherical bubble of the granule, the bubble rapidly recedes from the line of fusion, expanding to more than fifty times its former volume.

The diameter of the individual granule is from 0·0544 to 0·0724 millim., the diameter of the individual spherical body which is exactly in the centre of the granule, and which is seen to be a minute air-bubble, is 0·0088 millim. From what is observed on the melting of the granule, the bubble is air, yet air which is condensed to one-fiftieth of its original volume. The individual air-bubbles, which immediately after their expansion have under the ordinary atmospheric pressure a diameter of 0·02716 to 0·0314 millim., swim for a short time without change in the ice-water of the object-table. As the volumes of spheres are to each other as $r^3 : r_i^3$,

$$\frac{r^3}{r_i^3} = \frac{(0·0044)^3}{(0·0157)^3} = 1 : 52.$$

As according to Boyle's law $V : V_i = P_i : P$, in order to compress a bubble of air of the density of ordinary air from 0·0271 millim. diameter to a volume of 0·0088 diameter, a pressure of 52 atmospheres must have been exerted. Assuming that the hailstones were formed under the ordinary pressure, we may calculate the temperature at which a mass of air under the ordinary pressure would be contracted to 52 times as small a bulk. Since

$$V t_i = V t - V t \alpha t_i,$$

then

$$-t_i = \frac{V t_i - V t}{V t \alpha};$$

the value for the two volumes, reduced by four decimal places, is

$$-t_i = \frac{0·11304 - 0·00216}{0·11304 \times 0·00458} = -214° \text{ C}.$$

From this unusual degree of cold it is probable that the sole cause of condensation is not cold alone ; probably both causes have been operative in this enormous condensation of the air in the solid mass of ice.

Continual observation and comparison of many hailstones showed me that the condensed air-bubbles are in the middle of the spherical granule of the hailstone, and that the enclosed air which is disengaged from the melting hailstone is neither in the solid ice which fills the interstitial space between the individual granules, nor in the homogeneous solid mass of the granule. Refraining from any hints, I wish to direct the attention of observers to this interesting fact, and I recommend the subject to further observation.—Poggendorff's *Annalen*, No. 4, 1871.

XI. *On the Reflection of Light from Transparent Matter.* *By*
The Hon. J. W. Strutt, *M.A., Fellow of Trinity College,
Cambridge*[*].

IN connexion with other investigations on light I had occa-
sion to consider the problem of reflection, in order to see
how far the facts might be accounted for by the different hypo-
theses which have been made as to the condition of the æther in
transparent matter. Although, as I now find, some of the re-
sults then arrived at have been already given by Lorenz, of
Copenhagen, the publication of the present paper may not be
without use, as I cannot agree with him on many important
points, and great misapprehension seems to prevail on the sub-
ject generally.

Starting with the assumption that the rigidity is the same in
the two media, and that the vibrations of light are normal to the
plane of polarization, Fresnel was led to the conclusion that if
the incident vibration be represented by unity, the reflected
vibration is given by the expressions

$$\frac{\sin (\theta_{\prime}-\theta)}{\sin (\theta_{\prime}+\theta)}, \quad \frac{\tan (\theta_{\prime}-\theta)}{\tan (\theta_{\prime}+\theta)},$$

according to whether the plane of primitive polarization coincides
with or is perpendicular to the plane of incidence. The process
by which the first (sine) formula is obtained is rigorous, or at
least may be made so by additional explanations. With regard
to the second, the reasoning cannot be considered demonstrative,
although, as a matter of fact, the arbitrary principle assumed

[*] Communicated by the Author.

(that the vibrations in the two media* when resolved parallel to the surface of separation are equal) and the conclusion are approximately true.   Fresnel did not contemplate the possibility of a change of phase, as we now know from Jamin's experiments†, accompanies reflection in one, if not in both of the principal cases.

Green's important work " On the Laws of Reflection and Refraction of Light at the Common Surface of two non-crystallized Media," was read before the Cambridge Philosophical Society on December 11,1837, and published in the Transactions for 1838‡. In this paper, which has never received on the Continent the attention which it deserves, Green investigates the equations of motion of an elastic medium, setting out, as we should now say, from the principle of energy.   By Lagrange's method he deduces both the general equations applicable throughout the interior, and the conditions which must be satisfied at the surface of separation of two media.   The statical properties of an isotropic medium are defined by two constants A and B, the second expressing the *rigidity,* and the first depending, though not in the simplest manner, on the *compressibility.*   For the luminiferous æther it is shown that A must be indefinitely great, or that the medium resists change of volume with an infinite force.   During motion the inertia of the medium comes into play, and a constant expressing the density must be added to the two statical constants already mentioned.   In all this there seems to be nothing to which exception can be taken, unless it be to the assumption (expressly stated by Green) that the sphere of sensible action of the molecular forces, or, as I should prefer to say, the range of the mutual influence of the parts of the medium, is insensible in comparison with the length of the wave, and that the transition from the one state of things to the other at the bounding surface is so rapid that it may be treated as abrupt.

But in the application to the question of reflection further assumptions are made whose significance has been strangely misunderstood.   When light passes from air into a denser medium, it propagates itself slower than before in the ratio of $\mu : 1$, but this consideration alone is not sufficient to lead to a definite solution.   From the equation

$$\frac{B_{\iota}}{\rho_{\iota}} = \frac{1}{\mu^2} \frac{B}{\rho}$$

we can infer nothing as to the relation between B and B_{ι}, which

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* No account being taken of surface-waves.

† *Ann. de Chimie,* t. xxix. p. 31.

‡ Reprinted in ' Green's Mathematical Papers,' edited by Ferrers. MacMillan and Co. 1871.

must be known before further progress (other than tentative) can be made. From the fact that in all the known gases A is independent of the nature of the gas, Green argues that we may assume the same for B, " at least when we consider those phenomena only which depend merely on different states of the same medium, as is the case with light "—an inference which certainly appears very precarious. In a note he says, " Though for all known gases A is independent of the nature of the gas, perhaps it is extending the analogy rather too far to assume that in the luminiferous æther the constants A and B must always be independent of the state of the æther as found in different refracting substances. However, since the hypothesis greatly simplifies the equations due to the surface of junction of the two media, and is itself the most simple that could be selected, it seemed natural first to deduce the consequences which follow from it before trying a more complicated one, and, as far as I have yet found, these consequences are in accordance with observed facts."

In a very wild criticism of this theory, at the end of an otherwise sound paper [*], Kurz, having mistaken the meaning of A, B, attributes to Green the absurd assumption that the wave-velocities are the same in the two media, and metaphorically holds up his hands in amazement. I need hardly point out that Green's conditions $A = A_i$, $B = B_i$ are something quite different, and imply simply an identity of statical properties in the case of the two media. It may be shown, however, that the first $(A = A_i)$ is unnecessary, a fact which Green does not seem to have perceived. The cause of the refraction is a variation of the dynamical property (density). The rest of Green's reasoning is rigorous, admitting of no cavil. When the vibrations are normal to the plane of incidence, the amplitude of the reflected vibration is expressed accurately by Fresnel's sine-formula ; but the tangent-formula is only applicable to vibrations in the plane of incidence as a first approximation. It is evident that, in order that theory may at all agree with observation, the vibrations of light must be supposed to be performed normally to the plane of polarization ; indeed the two assumptions of constant rigidity and normal vibrations are closely bound up together in all parts of optics. The effect of the hypothetical relations $A = A_i$, $B = B_i$ is greatly to simplify the bounding conditions which then express the equality of the component displacements *and their derivatives* on the two sides of the separating surface. In this form they become identical with the so-called Principle of Continuity of Movement stated by Cauchy,

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[*] Pogg. *Ann.* vol. cviii. p. 396.

who does not appear to have seen that a continuity of strain implies necessarily a continuity of statical properties across the surface of separation, as is evident in a moment from D'Alembert's principle. So far there is absolute agreement between Green and Cauchy, the only difference being that Green went deeper into the matter and gave the interpretation, if not the justification of the principle assumed straight off by Cauchy. The divergence which exists between the results of the two theories takes its rise in their treatment of the longitudinal wave produced when the vibrations are in the plane of incidence, whose consideration cannot be dispensed with, although its direct effect is confined to within a few wave-lengths of the surface. Green merely supposes that the velocity of propagation of disturbances depending on change of volume is infinite in both media, and accordingly arrives at a result which contains only one constant—the refractive index; while Cauchy, on the other hand, imagines a sort of opacity to longitudinal vibrations, in virtue of which the waves are damped, and introduces a new constant called the coefficient of extinction. Cauchy, I believe, never published the proof of his formulæ; but the want has been supplied by German physicists*. Whatever may be thought of the processes by which they are obtained, there can be no doubt that Cauchy's formulæ agree very well with the observations of Jamin; while the same cannot be said of Green's as they stand in his original memoir. A modified form of the latter, however, has been given by Haughton †, to which I am inclined to adhere. He thought that, by supposing the incompressibility, though great, to be still finite, the second constant might be introduced, without which an agreement with observation is impossible. Apart from the difficulty of explaining what becomes of the longitudinal wave when the incidence is nearly normal, in which case it must be propagated in the ordinary way, his reasoning is entirely vitiated by an oversight already remarked on by Eisenlohr. The difference between Cauchy's formulæ and Green's, as modified by Haughton, is barely sensible in the experiments of Jamin, which are for the most part confined to the neighbhood of the polarizing angle; but according to Kurz ‡, whose observations extended over a wider range, the latter has a decided advantage as an empirical representation of the facts.

Quite different from the foregoing is the theory of Mac-Cullagh and Neumann, which is given in an accessible form in Lloyd's ' Wave-Theory of Light.' The following principles are laid down as the basis of investigation :—

* Beer, Pogg. *Ann.* vols. xci. and xcii.   Eisenlohr, Pogg. *Ann.* vol. civ. p. 346.
† Phil. Mag. S. 4. vol. vi. p. 81.   ‡ Pogg. *Ann.* vol. cviii.

I. The vibrations of polarized light are *parallel* to the plane of polarization.

II. The density of the æther is the same in all bodies as *in vacuo.*

III. The *vis viva* is preserved; from which it follows that the masses of the æther put in motion, multiplied by the squares of the amplitudes of vibration, are the same before and after reflection.

IV. The resultant of the vibrations is the same in the two media; and therefore in singly refracting media the refracted vibration is the resultant of the incident and reflected vibrations.

When the vibrations are normal to the plane of incidence, and therefore parallel in all three waves, the application of these principles gives rigorously Fresnel's tangent expression. If the vibrations are in the plane of incidence, the fourth principle alone leads to Fresnel's sine-formula. This only shows that the fourth principle is inconsistent with the others; for, as we shall see, unexceptionable reasoning founded on I. and II. leads to an altogether different result. The very particular case of IV. required when the vibrations are normal to the plane of incidence happens to be correct. In order to prevent misapprehension, I should say there is a sense in which IV. is perfectly true. If the vibrations belonging to the longitudinal surface-waves be included, it expresses merely the continuity of displacement, a condition which must necessarily be fulfilled according to any view of the subject. But understood in this true sense, it does not carry the consequences deduced from it. It remains then to be seen what the magnitude of the reflected wave would be according to principles I. and II., when the light is polarized in the plane of incidence. Let us take up the question after the method of Green, and inquire what are the consequences of the various suppositions which may be made: and first for light vibrating normally to the plane of incidence.

The plane of separation of the media being $x = 0$, let the axis of z be parallel to the fronts of the waves, so that $z = 0$ is the plane of incidence. The displacements in the two media are in general denoted by ξ, η, ζ; ξ_i, η_i, ζ_i; but in this case ξ, η, ξ_i, η_i all vanish. For the general equation of motion we have

$$\left.\begin{aligned}
\frac{d^2\zeta}{dt^2} &= \frac{n}{D}\left(\frac{d^2\zeta}{dx^2} + \frac{d^2\zeta}{dy^2}\right), \\
\frac{d^2\zeta_i}{dt^2} &= \frac{n'}{D'}\left(\frac{d^2\zeta_i}{dx^2} + \frac{d^2\zeta_i}{dy^2}\right);
\end{aligned}\right\} \quad \cdots \quad (1)$$

and for the bounding conditions,

$$\left.\begin{array}{c} \zeta=\zeta_{\prime}, \\ n\dfrac{d\zeta}{dx}=n'\dfrac{d\zeta_{\prime}}{dx}, \end{array}\right\} \quad \text{when } x=0 \; ; \quad . \quad . \quad . \quad (2)$$

n, n' are the rigidities; D, D' the densities.

Assume

$$\zeta=f(ax+by+ct)+\mathrm{F}(-ax+by+ct),$$
$$\zeta_{\prime}=f_{\prime}(a_{\prime}x+by+ct),$$

the coefficients b and c being necessarily the same for all three waves, since their traces on the surface must move together. Hence from (2)

$$\left.\begin{array}{c} f'+\mathrm{F}'=f_{\prime}', \\ n(af'-a\mathrm{F}')=n'a_{\prime}f_{\prime}', \end{array}\right\} \quad *$$

and

$$\frac{\mathrm{F}'}{f'}=\frac{\dfrac{a}{a_{\prime}}-\dfrac{n'}{n}}{\dfrac{a}{a_{\prime}}+\dfrac{n'}{n}};$$

or, since $\dfrac{b}{a}=\tan\theta$, $\dfrac{b}{a_{\prime}}=\tan\theta_{\prime}$,

$$\frac{\mathrm{F}'}{f'}=\frac{\dfrac{\tan\theta_{\prime}}{\tan\theta}-\dfrac{n'}{n}}{\dfrac{\tan\theta_{\prime}}{\tan\theta}+\dfrac{n'}{n}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

an equation giving the ratio of the reflected and incident vibrations.

Case I. (Green's) $n=n'$:

$$\frac{\mathrm{F}'}{f'}=\frac{\cot\theta-\cot\theta_{\prime}}{\cot\theta+\cot\theta_{\prime}}=\frac{\sin(\theta_{\prime}-\theta)}{\sin(\theta_{\prime}+\theta)}.$$

Case II. (MacCullagh's) D = D'.

Since generally

$$\frac{n}{\mathrm{D}}:\frac{n'}{\mathrm{D}'}=\mu^2:1,$$

we have

$$\frac{n'}{n}=\frac{1}{\mu^2}=\frac{\sin^2\theta_{\prime}}{\sin^2\theta},$$

and then (3) gives

$$\frac{\mathrm{F}'}{f'}=\frac{\tan(\theta_{\prime}-\theta)}{\tan(\theta_{\prime}+\theta)}.$$

If we assume the complete accuracy of Fresnel's expressions, either case agrees with observation; only, if $n = n'$, light vibrates normally to the plane of polarization; while if $D = D'$, the vibrations are parallel to that plane. But we know that Fresnel's tangent-formula is not accurate, and that there is in general no angle of complete polarization, so that already the presumption is in favour of Case 1; but I would not lay much stress upon this, as the phenomena investigated by Jamin are of a secondary character, and might be due to the action of disturbing causes.

Case III. We may suppose that n and D both vary. Here we should obtain something between Fresnel's two expressions, which could hardly be reconciled with observation, unless one variation were very subordinate to the other. Other considerations seem to exclude this case; for if n and D both vary, there is nothing to prevent their varying proportionally, so as to leave the wave-velocity unchanged, or $\mu = 1$. The transmitted wave would then not be turned, although there would be a finite reflection. Nothing of this kind is known in nature, whichever way the light may be polarized. But the most satisfactory argument against the joint variation is derived from the theory of the diffraction of light from very small particles, whose diameter does not exceed a small fraction of the wave-length. Hitherto there has been no theoretical difficulty. Case I. is only a translation into analysis of the reasoning of Fresnel, and Case II. of the reasoning of MacCullagh. But when we pass on to the consideration of the problem when the vibrations are in the plane of incidence, our footing is no longer so sure. However close the analogy may be between the phenomena of light and the transverse vibrations of an elastic solid, one cannot but feel that it may not extend to those motions which are independent of rigidity, and of which in the case of the æther we have no direct knowledge. Still, in the absence of all others, we cannot do better than follow the guide which has already served us so well.

Since the displacement is entirely in the plane of incidence, $\zeta = 0$, and ξ, η are independent of z. The equations to be satisfied in the interior of the first medium are*,

$$\left. \begin{array}{l} \dfrac{d^2\xi}{dt^2} = g^2 \dfrac{d}{dx}\left(\dfrac{d\xi}{dx} + \dfrac{d\eta}{dy}\right) + \gamma^2 \dfrac{d}{dy}\left(\dfrac{d\xi}{dy} - \dfrac{d\eta}{dx}\right), \\[2ex] \dfrac{d^2\eta}{dt^2} = g^2 \dfrac{d}{dy}\left(\dfrac{d\xi}{dx} + \dfrac{d\eta}{dy}\right) + \gamma^2 \dfrac{d}{dx}\left(\dfrac{d\xi}{dy} - \dfrac{d\eta}{dx}\right), \end{array} \right\} \quad . \quad (4)$$

where

$$g^2 = \frac{m+n}{D}, \quad \gamma^2 = \frac{n}{D}.$$

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* See Green, or Thomson and Tait, p. 530.

Putting, with Green,

$$\left.\begin{array}{l} \xi = \dfrac{d\phi}{dx} + \dfrac{d\psi}{dy}, \\[2mm] \eta = \dfrac{d\phi}{dy} - \dfrac{d\psi}{dx}, \end{array}\right\} \qquad . \quad . \quad . \quad . \quad . \quad (5)$$

we find

$$\left.\begin{array}{l} \dfrac{d^2\phi}{dt^2} = g^2\left(\dfrac{d^2\phi}{dx^2} + \dfrac{d^2\phi}{dy^2}\right), \\[2mm] \dfrac{d^2\psi}{dt^2} = \gamma^2\left(\dfrac{d^2\psi}{dx^2} + \dfrac{d^2\psi}{dy^2}\right). \end{array}\right\} \qquad . \quad . \quad . \quad . \quad (6)$$

Two similar equations apply to the lower medium.

The boundary conditions are

$$\xi = \xi_{\prime}, \quad \eta = \eta_{\prime},$$

$$(m+n)\frac{d\xi}{dx} + (m-n)\frac{d\eta}{dy} = (m'+n')\frac{d\xi_{\prime}}{dx} + (m'-n')\frac{d\eta_{\prime}}{dy},$$

$$n\left(\frac{d\xi}{dy} + \frac{d\eta}{dx}\right) = n'\left(\frac{d\xi_{\prime}}{dy} + \frac{d\eta_{\prime}}{dx}\right);$$

of which the first pair express the continuity of displacement, and the second the continuity of *stress.* Assume

$$\left.\begin{array}{l} \psi = \psi' \epsilon^{i(ax+by+ct)} + \psi'' \epsilon^{i(-ax+by+ct)}, \\[1mm] \phi = \phi \epsilon^{i(a'x+by+ct)}, \end{array}\right\} \text{upper medium};$$

$$\left.\begin{array}{l} \psi_{\prime} = \psi_{\prime} \epsilon^{(i(a_{\prime}x+by+ct))}, \\[1mm] \phi_{\prime} = \phi_{\prime} \epsilon^{i(a_{\prime}'x+by+ct)}, \end{array}\right\} \text{lower medium}.$$

The coefficient of t must be the same for all the waves on account of the periodicity, and b must be the same because the traces of all the waves on the plane of separation $x = 0$ must move together. The constants ψ', ψ'', ... are complex. From (6) we get the following relations,

$$c^2 = \gamma^2(a^2 + b^2) = \gamma_{\prime}^2(a_{\prime}^2 + b^2) = g^2(a'^2 + b^2) = g_{\prime}^2(a_{\prime}'^2 + b^2).$$

Since g and $g_{\prime}$ are indefinitely great,

$$a'^2 + b^2 = 0, \quad a_{\prime}'^2 + b^2 = 0; \quad . \quad . \quad . \quad . \quad (7)$$

whence we obtain

$$a' = ib, \quad a_{\prime}' = -ib$$

if the upper medium correspond to the positive x. Equations (7) express the incompressibility of the æther in the two media,

for

$$\delta = \frac{d\xi}{dx} + \frac{d\eta}{dy} = \frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = -(a'^2 + b^2)\phi.$$

It is therefore hardly correct to call the surface-waves expressed by ϕ *longitudinal.* They are more allied to those motions with which we have so much to do in hydrodynamics, which involve neither rotation nor yet change of volume.

Since $\dfrac{b}{a} = \tan\theta$; $\dfrac{b}{a_i} = \tan\theta_i$,

$$\frac{a^2 + b^2}{a_i^2 + b^2} = \frac{\sin^2\theta_i}{\sin^2\theta} = \frac{\gamma_i^2}{\gamma^2} = \frac{1}{\mu^2},$$

which expresses the ordinary law giving the direction of the refracted wave.

We have now to satisfy the boundary conditions. From the continuity of displacement,

$$\left. \begin{array}{l} a'\phi + b(\psi' + \psi'') = a_i'\phi_i + b\psi_i, \\ b\,\phi - a(\psi' - \psi'') = b\,\phi_i - a_i\psi_i\,; \end{array} \right\}$$

or, on introducing the values of a', a_i', and putting $\psi' + \psi'' = X$, $\psi' - \psi'' = Y$,

$$\left. \begin{array}{l} i(\phi + \phi_i) = \psi_i - X, \\ b(\phi - \phi_i) = aY - a_i\psi_i. \end{array} \right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Were we to ignore the surface-waves altogether and put $\phi = \phi_i = 0$, equations (8) would give us

$$X = \psi_i, \quad Y = \frac{a_i}{a}\psi_i;$$

whence

$$\frac{\psi''}{\psi'} = \frac{X - Y}{X + Y} = \frac{1 - \dfrac{a_i}{a}}{1 + \dfrac{a_i}{a}} = \frac{\sin(\theta_i - \theta)}{\sin(\theta_i + \theta)},$$

Fresnel's first expression. This is exactly what has been done by Zech[*], and is in fact merely a translation into analysis of MacCullagh's fourth principle. The worthlessness of the argument is sufficiently shown by the consideration that no assumption has yet been made as to the relations between n, n', D, D', other than that implied in taking the ratio of the wave-velocities equal to μ. It is as necessary to satisfy the second pair of boundary conditions, expressing the continuity of stress, as the

[*] Pogg. *Ann.* vol. cix. p. 60.

first; and this cannot be done without the introduction of finite surface-waves. Expressed in terms of ϕ, ψ, they take the form

$$(m+n)\frac{d^2\psi}{dx^2} + (m-n)\frac{d^2\phi}{dy^2} + 2n\frac{d^2\psi}{dx\,dy} = \text{similar expression,}$$

$$n\left\{2\frac{d^2\phi}{dx\,dy} + \frac{d^2\psi}{dy^2} - \frac{d^2\psi}{dx^2}\right\} = \text{similar expression;}$$

or on substitution of the values of ϕ, ψ, with regard to (8),

$$\phi\{m(a'^2+b^2)-2nb^2\} + 2nab\,\mathrm{Y}$$
$$= \phi_{,}\{m'(a_{,}'^2+b^2)-2n'b^2\} + 2n'a_{,}b\psi_{,}, \quad . \quad . \quad (9)$$

$$n\{b^2\psi_{,}-a^2\mathrm{X}+ib(a\mathrm{Y}-a_{,}\psi_{,})\}$$
$$= n'\{b^2\mathrm{X}-a_{,}^2\psi_{,}+ib(a\mathrm{Y}-a_{,}\psi_{,})\}. \quad . \quad . \quad (10)$$

Although a'^2+b^2 is vanishingly small, we are not at liberty to leave it out, because $m(a'^2+b^2)$ is finite. In fact

$$\left.\begin{array}{c} m(a'^2+b^2)=\mathrm{D}c^2, \\ m'(a_{,}'^2+b^2)=\mathrm{D}'c^2, \end{array}\right\} \quad . \quad . \quad . \quad . \quad (11)$$

for we may neglect n in comparison with m. Using these we obtain

$$\mathrm{D}\phi-\mathrm{D}'\phi_{,}= \frac{b^2(n-n')}{c^2}\left\{\frac{\psi_{,}-\mathrm{X}}{i} - \frac{a\mathrm{Y}+a_{,}\psi_{,}}{b}\right\}. \quad . \quad (9')$$

Equations (8), (9′), and (10) contain the solution of the problem.

Case 1. Let $n=n'$; (9′) and (10) give

$$\left.\begin{array}{l} \mathrm{D}\phi=\mathrm{D}'\phi_{,}, \\ \mathrm{X}(a^2+b^2)=\psi_{,}(a_{,}^2+b^2). \end{array}\right\}$$

Now $\mathrm{D}'\!:\mathrm{D}=\mu^2$; so that, from (8),

$$\mu^2\left\{\frac{\psi_{,}-\mathrm{X}}{i} - \frac{a\mathrm{Y}-a_{,}\psi_{,}}{b}\right\} = \frac{\psi_{,}-\mathrm{X}}{i} + \frac{a\mathrm{Y}-a_{,}\psi_{,}}{b};$$

or since

$$\mathrm{X}=\frac{a_{,}^2+b^2}{a^2+b^2}\,\psi_{,}=\mu^2\psi_{,}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

$$\mathrm{Y}=\left\{\frac{a_{,}}{a}+i\frac{b}{a}\frac{(\mu^2-1)^2}{\mu^2+1}\right\}\psi_{,}$$

$$=\left\{\frac{\cot\theta_{,}}{\cot\theta}+i\tan\theta\mathrm{M}(\mu^2-1)\right\}\psi_{,}, \quad . \quad . \quad . \quad (13)$$

if we put

$$\frac{\mu^2-1}{\mu^2+1}=\mathrm{M}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

From (13) and (14),

$$2\psi' = X + Y = \left\{ \mu^2 + \frac{\cot\theta_l}{\cot\theta} + i\tan\theta M(\mu^2 - 1) \right\} \psi_l,$$

$$2\psi'' = X - Y = \left\{ \mu^2 - \frac{\cot\theta_l}{\cot\theta} - i\tan\theta \, M(\mu^2 - 1) \right\} \psi_l.$$

The quantities within the brackets are complex, and may be exhibited in the forms $R\epsilon^{ie}$, $R'\epsilon^{ie'}$; e and e' then denote the difference of phase between the incident and refracted, the reflected and refracted waves respectively, and are given by

$$\cot e = \frac{1}{M(\mu^2 - 1)} \{\mu^2 \cot\theta + \cot\theta_l\}$$

$$= \frac{1}{M} \cot(\theta - \theta_l); \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

by trigonometrical transformation, with use of relation

$$\sin\theta = \mu\sin\theta_l;$$

$$\cot e' = \frac{1}{M(\mu^2 - 1)} \{-\mu^2\cot\theta + \cot\theta_l\}$$

$$= -\frac{1}{M} \cot(\theta + \theta_l). \quad . \quad . \quad . \quad . \quad . \quad (16)$$

We have seen that when the vibrations are normal to the plane of incidence there is no difference of phase between the incident and reflected waves, unless the change of sign, when the second medium is the denser, be considered such. Now what is observed in experiments is the acceleration or retardation of the one polarized component with regard to the other, and is therefore given simply by $e - e'$. The ambiguity must be removed by the consideration that when the incidence is normal there is no relative change of phase, though throughout Jamin's papers it is assumed that there is in that case a phase-difference of half a period. I am at a loss to understand how Jamin could have entertained such a view, which is inconsistent with continuity, inasmuch as when $\theta = 0$ the distinction between polarization in the plane of reflection and polarization in the perpendicular plane disappears.

The ratio of the amplitudes of the reflected and incident vibrations is given by

$$\frac{R'^2}{R^2} = \frac{(-\mu^2\cot\theta + \cot\theta_l)^2 + M^2(\mu^2 - 1)^2}{(\mu^2\cot\theta + \cot\theta_l)^2 + M^2(\mu^2 - 1)^2}$$

$$= \frac{\cot^2(\theta + \theta_l) + M^2}{\cot^2(\theta - \theta_l) + M^2}. \quad . \quad . \quad . \quad . \quad . \quad (17)$$

The corresponding quantity when the light is polarized in the plane of incidence is

$$\frac{\mathrm{R}''^2}{\mathrm{R}^2} = \frac{\sin^2(\theta - \theta_{\prime})}{\sin^2(\theta + \theta_{\prime})},$$

and therefore

$$\frac{\mathrm{R}'^2}{\mathrm{R}''^2} = \frac{\cos^2(\theta + \theta_{\prime}) + \mathrm{M}^2 \sin^2(\theta + \theta_{\prime})}{\cos^2(\theta - \theta_{\prime}) + \mathrm{M}^2 \sin^2(\theta - \theta_{\prime})}. \quad \ldots \quad (18)$$

Equations (14), (15), (16), (17), (18) constitute the solution of the problem on the hypothesis that $n = n'$, and are equivalent to results given by Green.

Case 2. Let $\mathrm{D} = \mathrm{D}'$; $n' : n = 1 : \mu^2$. (9′) and (10) assume the form

$$\mu^2(a^2 + b^2)(a\mathrm{Y} - a_{\prime}\psi_{\prime}) = b^2(\mu^2 - 1)\left\{\frac{b}{i}(\psi_{\prime} - \mathrm{X}) - a\mathrm{Y} - a_{\prime}\psi_{\prime}\right\},$$

$$\mu^2(b^2\psi_{\prime} - a^2\mathrm{X}) - b^2\mathrm{X} + a_{\prime}^2\psi_{\prime} = -(\mu^2 - 1)ib(a\mathrm{Y} - a_{\prime}\psi_{\prime}),$$

the value of $\phi - \phi_{\prime}$ being substituted from (8), or, on expressing a, b, &c. in terms of the angles of incidence and refraction,

$$\cot\theta\mathrm{Y} - \cot\theta_{\prime}\psi_{\prime} = \frac{\mu^2 - 1}{\mu^2}\sin^2\theta\left\{\frac{\psi_{\prime} - \mathrm{X}}{i} - \cot\theta\mathrm{Y} - \cot\theta_{\prime}\psi_{\prime}\right\},$$

$$\mu^2(\psi_{\prime} - \cot^2\theta\mathrm{X}) - \mathrm{X} + \cot^2\theta_{\prime}\psi_{\prime} = -i(\mu^2 - 1)(\cot\theta\mathrm{Y} - \cot\theta_{\prime}\psi_{\prime}).$$

From these two equations the values of X and Y as functions of the angle of incidence might be tabulated with any given value of μ. One particular case is very remarkable. At the polarizing angle $(\tan^{-1}\mu)$ the *amplitude* of the reflected wave is the same as it would be given by Fresnel's sine-formula—a coincidence for which I have not been able to see any reason.

My object in bringing forward the present hypothesis is to disprove it, and is sufficiently attained by the disproof of a particular case. Let us therefore suppose that the difference of refrangibility between the two media is so small that the square and higher powers of $(\mu^2 - 1)$ may be neglected. In the small terms we are to put

$$\mathrm{X} = \mathrm{Y} = \psi_{\prime}, \quad \cot\theta = \cot\theta_{\prime}.$$

The second equation gives

$$\mathrm{X} = \frac{\mu^2 + \cot^2\theta_{\prime}}{1 + \mu^2\cot^2\theta}\psi_{\prime},$$

while from the first

$$\mathrm{Y} = \frac{\cot\theta_{\prime}}{\cot\theta}\psi_{\prime} - 2\frac{\mu^2 - 1}{\mu^2}\sin^2\theta\psi_{\prime}.$$

Now $\cot^2 \theta_l = \dfrac{\mu^2 - 1}{\sin^2 \theta} + \cot^2 \theta,$

$$\cot \theta_l = \cot \theta \left(1 + \frac{\mu^2 - 1}{2 \cos \theta}\right) \text{ approx.}$$

Hence

$$X = \{1 + 2(\mu^2 - 1) \sin^2 \theta\} \, \psi_l,$$

$$Y = \left\{ 1 + (\mu^2 - 1)\left[\frac{1}{2 \cos^2 \theta} - 2 \sin^2 \theta\right] \right\} \psi_l,$$

$$\frac{\psi''}{\psi'} = \frac{X - Y}{X + Y} = (\mu^2 - 1)\frac{2 \sin^2 2\theta - 1}{4 \cos^2 \theta} = -\frac{\mu^2 - 1}{2}\frac{\cos 4\theta}{1 + \cos 2\theta}. \quad (19)$$

From (19) we see that the reflected wave vanishes when $\cos 4\theta = 0$; that is, when

$$\theta = \frac{\pi}{8}, \text{ or } \theta = \frac{3\pi}{8}.$$

It appears, then, that on the hypothesis $D = D'$, there would be two polarizing angles $(\frac{\pi}{8}, \frac{3\pi}{8}$ respectively) whenever the difference of refrangibility between the two media is small. Since nothing of the sort is observed, we conclude that D cannot be equal to D′, and are driven to adopt Green's original view that the rigidity of the æther is the same in all media.

Results substantially equivalent to (19) have been already given in a different form by Lorenz[*], who, however, has not discussed them, but simply says that they cannot be reconciled with Fresnel's formulæ. Curiously enough he has taken the same particular case for disproof which I, without a knowledge of his work, had hit upon. Those who have done me the honour of reading my papers on the action of small particles on light will understand how I anticipated the two polarizing angles by the very different process there employed. Lorenz draws the conclusion that the elastic force of the æther is the same in all transparent uncrystalline substances as *in vacuo*, and that the vibrations of light are performed normally to the plane of polarization. He might, I think, have omitted the word *uncrystalline*[†].

There is also another paper[‡] by Lorenz on this subject, in which he endeavours to account for the correction to Fresnel's tangent-formula required by experiment, by supposing that the transition from the one medium to the other, instead of being

[*] Pogg. *Ann.* vol. cxiv.   [†] Phil. Mag. S. 4. vol. xli. p. 519.
[‡] Pogg. *Ann.* vol. cxi.

sudden as we have hitherto considered it, occupies a distance not immeasurably less than the wave-length,—certainly a very reasonable supposition. But there are two objections to his view which are, to my mind, fatal. In the first place, Fresnel's tangent-formula does not express the result of a sudden transition; and what is more, Green's formula (17), which does express it, deviates from the truth on the other side. The difficulty is not to explain why Fresnel's formula is not accurately correct, but why the divergences from it are not greater than we actually find them. According to (17), the light reflected at the polarizing angle from the diamond or any other substances of high refractive index would be a very considerable fraction of the whole, very much greater than what is observed. Another objection to the view that the light reflected at the polarizing angle is due to the want of abruptness in the transition, seems to be contained in the consideration that, if this were really its origin, it ought to show a colour corresponding to the blue of the first order in Newton's scale, being to all intents and purposes reflected from a thin plate. Observation, so far as I am aware, gives no support to such an idea.

Cauchy's formulæ, which differ from (15), (16), (17) merely by the substitution of $-\epsilon \sin \theta$ for M, agree very well with experiment; but I cannot regard them as having a sound dynamical foundation. The introduction of evanescent waves of the kind used by Cauchy involves, as Lorenz remarks, a theory of imperfectly elastic media. But the case is even worse than this; for it may, I believe, be shown that no reasonable theory could lead to the peculiar form of evanescence assumed by Cauchy. Let us examine this point.

If, in the investigation of Cauchy's formulæ as given by Beer, we introduce the functions ϕ and ψ used by Green, we find that ϕ is still expressed by an exponential function of the same form as before, viz $\epsilon^{i(a'x+by+ct)}$. The only difference is that, whereas in Green's theory $a'^2+b^2=c^2 \div g^2$, the relation between a', b, c, according to Cauchy, is

$$a'^2 + b^2 = -k^2, \quad . \quad . \quad . \quad . \quad . \quad (20)$$

where k is the so-called coefficient of extinction. The working out is nearly the same as before. Instead of (8) we have

$$\left.\begin{array}{l} a'\phi - a_i'\phi_i = b(\psi_i - \mathrm{X}), \\ b(\phi - \phi_i) = a\mathrm{Y} - a_i\psi_i. \end{array}\right\} \quad . \quad . \quad . \quad (21)$$

Again, since, according to Cauchy's principle, $m'=m_i,'\ n'=n$, $(9')$ becomes, in virtue of (20),

$$k^2\phi = k_i^2\phi_i, \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

(10) is replaced by

$$2a'b\phi + (b^2 - a^2)X = 2a_l'b\phi_l + (b^2 - a_l^2)\psi_l,$$

or, by (21),

$$X = \frac{a_l^2 + b^2}{a^2 + b^2}\psi_l = \mu^2\psi_l. \quad \ldots \quad \ldots \quad (23)$$

From (21), (22),

$$\frac{Y}{\psi_l} = \frac{a_l}{a} - \frac{b^2(\mu^2 - 1)}{a}\frac{k^2 - k_l^2}{a_l'k^2 - a'k_l^2}. \quad \ldots \quad (24)$$

From (24) we may fall back on Green's corresponding equation (13) by putting $k = 0$, $k_l = 0$, $k_l : k = \mu : 1$; but Cauchy supposes, on the contrary, that k^2, k_l^2 are very large in comparison with b^2, and writes

$$a' = ik, \quad a_l' = -ik,$$

which convert (24) into

$$\frac{Y}{\psi_l} = \frac{a_l}{a} + i\frac{(\mu^2 - 1)b^2}{a}\left(\frac{1}{k} - \frac{1}{k_l}\right).$$

Cauchy further takes

$$\frac{2\pi}{\lambda}\left(\frac{1}{k} - \frac{1}{k_l}\right) = -\epsilon;$$

so that, since $\frac{2\pi}{\lambda}\sin\theta = b$, the solution of the problem is

$$\left.\begin{array}{l} X = \mu^2\psi_l, \\[4pt] Y = \left\{\dfrac{a_l}{a} - i(\mu^2 - 1)\dfrac{b}{a}\epsilon\sin\theta\right\}\psi_l. \end{array}\right\} \quad \ldots \quad (25)$$

It may, however, be remarked that Cauchy has no right to suppose that ϵ is a constant for the rays of different wave-lengths. In fact if k and k_l are constants, ϵ varies inversely as λ; so that the same objection arises here as in the theory of Lorenz. The only difference between (25) and (12), (13) lies in the substitution of $-\epsilon\sin\theta$ for M. It is therefore unnecessary to write down the results corresponding to (15), (16), (17), (18).

But what I wish particularly to point out is the extraordinary differential equation satisfied by ϕ. By differentiating the expression for ϕ and substitution in (20), we find

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = -\frac{k^2}{c^2}\frac{d^2\phi}{dt^2}.$$

I am at a loss to understand how any mechanical theory of imperfect elasticity could lead to such an equation. If we were to speculate as to the most probable form of the equation of motion, we should perhaps give the preference to

$$\frac{d^2\phi}{dt^2} + h\frac{d\phi}{dt} = \varrho^2\left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2}\right);$$

but the form of ϕ so determined is different from Cauchy's, and leads to a more complicated solution. On the whole I cannot see that Cauchy's theory of reflection has any claim to be considered dynamical, although his formulæ are, beyond doubt, very good empirical representations of the facts.

I now come to the modification of Green's theory proposed by Haughton. If M were an arbitrary constant instead of a definite function of μ, there would be but little difference between the two sets of formulæ; for the factor $\sin \theta$ would not vary greatly in the neighbourhood of the polarizing angle, where alone the correction to Fresnel's original expression is sensible. So far as the question has been treated experimentally, the balance of evidence seems to be rather against than for the factor $\sin \theta$. I have already remarked that Haughton's reasons for considering M as an independent constant cannot be sustained, but at the same time I think that others of considerable force may be given.

In a supplement to his memoir "On the Reflection of Light"*, Green says :—" Should the radius of the sphere of sensible action of the molecular forces bear any finite ratio to λ, the length of a wave of light, as some philosophers have supposed in order to explain the phenomena of dispersion, instead of an abrupt termination of our two media we should have a continuous though rapid change of state of the ætherial medium in the immediate vicinity of their surface of separation. And I have here endeavoured to show by probable reasoning that the effect of such a change would be to diminish greatly the quantity of light reflected at the polarizing angle, even for highly refractive substances, supposing the light polarized perpendicular to the plane of incidence." The contrast between this view and that of Lorenz is remarkable.

Referring to equation (9), we see that when $n'=n$, it reduces to

$$m(a'^2 + b^2)\phi = m'(a_1'^2 + b^2) \phi_1.$$

Reasoning from the analogy of elastic solids, we found

$$m(a'^2 + b^2) : m'(a_1'^2 + b^2) = \mathrm{D} : \mathrm{D}'. \quad . \quad (11)$$

Now although the transition between the two media is so sudden that the principal waves of transverse vibrations are affected nearly in the same way as if it were instantaneous, yet we may readily imagine that the case is different for the surface-waves, whose existence is almost confined to the layer of variable density. It is probable that the ratio of $m(a'^2 + b^2) : m'(a_1'^2 + b^2)$, instead of being equal to $1 : \mu^2$, approaches much more nearly to

* Cambridge Trans. 1839, or Green's works.

a ratio of equality. We may therefore take

$$\phi : \phi_i = \mu_0^2 : 1,$$

where μ_0^2 is less than μ^2. The solution is the same as before, except that now

$$M = \frac{\mu_0^2 - 1}{\mu_0^2 + 1}.$$

This explanation of the deviation of M from Green's value seems to me the most probable; but the ground might be taken that the densities concerned in the propagation of the so-called longitudinal waves are unknown, and may possibly not be the same as those on which transverse vibrations depend. For sulphuret of arsenic, Jamin's experiments give

$$\mu = 2 \cdot 454, \quad \mu_0 = 1 \cdot 083,$$

showing that μ_0 is very considerably less than μ.

One of the most remarkable of Jamin's results shows that in many cases M is negative, or μ_0 less than unity. There are a few substances of an intermediate character for which $M = 0$; and then Fresnel's original formulæ express the laws of the phenomena. The value of μ is usually about $1 \cdot 45$. No adequate explanation has hitherto been given of the singular law; and in the remarks which follow I wish to be understood as merely throwing out a suggestion which may or may not contain the germ of an explanation.

It is known that many solid bodies have the power of condensing gases on their surfaces, a property on which the action of Grove's gas-battery seems to depend. Now, if we were to suppose that at the surfaces of solid and liquid bodies there exists a sheet of condensed air, which need not extend to a distance greater than the wave-length, but is of an optical density corresponding to about $\mu = 1 \cdot 5$, the occurrence of negative values of M would, I think, be explained. There is nothing *à priori* very improbable in the existence of such a sheet, so far as I am able to see; but it is for experiment to decide whether the phenomena observed near the polarizing angle depend in any manner on the nature of the gas with which the reflecting body is in contact, and whether the sign of M may change from negative to positive when vacuum is substituted for atmospheric air. The fact that the value of M for the surface of separation of (say) glass and water cannot be calculated from the values of M corresponding respectively to glass and air, water and air, seems to indicate that the phenomenon is, so to speak, of an accidental character.

XII. *On* Mr. Hopkins's *Method of determining the Thickness of the Earth's Crust.* *By* Archdeacon Pratt, *M.A., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN my letter in your Number for July of last year I gave a reply to M. Delaunay on the above subject in a popular form,—and in that way explained that, even if the fluid interior of the earth's mass at any moment revolved with the crust as if the two were one solid mass, this state could not continue; the crust under the action of the precessional force would slip over the fluid, not being solidly connected with it.

M. Delaunay (as reported in 'Nature,' March 16, 1871, column 1) has again said that "calculations prove" that the thickness of the crust has "no influence on the revolution of the earth." I therefore now send you a calculation to show that the crust, with an interior fluid nucleus, both following the law of density adopted by Laplace, cannot move as it would if the crust and nucleus were one solid mass. The method I pursue is this. At the epoch from which t (the time) is measured I assume that things are exactly as M. Delaunay supposes, viz. that internal friction and viscosity have reduced the fluid to entire obedience at that moment to the movements of the crust. I then show by the equations that this state of things cannot continue. This mode of taking the problem enables me easily to calculate the effect of the fluid pressure on the crust at the epoch; and any minute motion, gradually generated in the fluid after this for a short time, would enter into the equations as a quantity of the second and higher orders and may be neglected. The slowness of the disturbed motion and the viscosity would have the effect of prolonging the period through which my equations would apply.

2. The forces acting on the crust are the attraction of the sun and moon from without, and the pressure of the fluid against its interior surface; and the pressure of the fluid is produced by the centrifugal force and the attraction of the sun and moon on the fluid. As the crust is supposed to be made up of spheroidal shells, it will have no effect on the fluid.

Let ω_1, ω_2, ω_3 be the angular velocities of the solid crust round any line at right angles to the earth's axis, another axis at right angles to it and lying in the plane of the equator, and the axis of rotation; *i. e.* the axes of x, y, z fixed in the body; A, A, C the moments of inertia about those three axes; L, M the moments of the forces acting on the crust about the axes of x and y. There will be no moment of forces about z, because the resultant effect of each set of forces passes through the axis

of z, owing to the symmetry of figure. Hence

$$A\frac{d\omega_1}{dt} + (C-A)\omega_3\omega_2 = L,$$

$$A\frac{d\omega_2}{dt} - (C-A)\omega_3\omega_1 = M,$$

$$C\frac{d\omega_3}{dt} \qquad = 0*.$$

The third equation gives $\omega_3 = a$, constant $= n$; and the others become

$$A\frac{d\omega_1}{dt} + (C-A)n\omega_2 = L,$$

$$A\frac{d\omega_2}{dt} - (C-A)n\omega_1 = M.$$

3. The values of L and M for the sun and moon are known from the ordinary problem of precession and nutation; they are easily shown to be

$$(C-A)\frac{3S}{c^3}\sin\theta\cos\theta\sin\phi, \quad -(C-A)\frac{3S}{c^3}\sin\theta\cos\theta\cos\phi$$

for the sun, where S is the sun's mass, c his mean distance, θ and ϕ his colatitude and right ascension†. Similar expressions are true for the moon's action. Let them be

$$(C-A)\frac{3M}{c_l^3}\sin\theta_l\cos\theta_l\sin\phi_l, \quad -(C-A)\frac{3M}{c_l^3}\sin\theta_l\cos\theta_l\cos\phi_l.$$

4. I will now find L and M for the pressure of the fluid. Let r, θ', ϕ' be the coordinates to any point in the inner surface of the crust, and p the fluid pressure. Then $pr^2\sin\theta'd\phi'd\theta'$ is the pressure on an element of the surface, and acts in the normal. Let l be the angle the normal makes with the earth's axis. Then by conics

$$l = \theta' - 2\epsilon\cos\theta'\sin\theta',$$

ϵ being the ellipticity of the inner surface of the crust. Hence the pressures parallel to the axes are

$$pr^2\sin\theta'd\phi'd\theta'.\sin l\cos\phi', \quad pr^2\sin\theta'd\phi'd\theta'.\sin l\sin\phi',$$

and

$$pr^2\sin\theta'd\phi'd\theta'.\cos l;$$

also

$$x = r\sin\theta'\cos\phi', \quad y = r\sin\theta'\sin\phi', \quad z = r\cos\theta'.$$

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* These equations will be found in the *Mécanique Céleste*, or any work on the motion of a rigid body. I have taken them from my 'Mechanical Philosophy,' second edition, p. 425, making $B=A$.

† Mechanical Philosophy, p. 426.

Hence the moment of the fluid pressure on this element about the axis of x

$$= pr^3 \sin \theta' d\phi' d\theta' (\cos l \cdot \sin \theta' \sin \phi' - \sin l \sin \phi' \cdot \cos \theta)$$
$$= pr^3 \sin \theta' d\phi' d\theta' \sin \phi' \cos (l - \theta') = 2\epsilon p a^3 \sin^2 \theta' \cos \theta' \sin \phi' d\theta' d\phi',$$

putting $r = a$, the mean radius, because the square of ϵ may be neglected. Integrating for the whole surface, putting $\cos \theta' = \mu'$, the part of L which depends on the fluid pressure

$$= 2a^3 \epsilon \int_{-1}^{1} \int_{0}^{2\pi} p \mu' \sqrt{1 - \mu'^2} \sin \phi' d\mu' d\phi'.$$

Similarly the part of the moment M which depends on fluid pressure

$$= -2a^3 \epsilon \int_{-1}^{1} \int_{0}^{2\pi} p \mu' \sqrt{1 - \mu'^2} \cos \phi' d\mu' d\phi'.$$

The function under the signs of integration is a Laplace's function of the second order.

5. I must now find p. The centrifugal forces on any particle $(x'y')$ of the fluid parallel to x and y are $n^2 x'$ and $n^2 y'$. Also, if R is the distance of the sun from that particle, the attraction of the sun on it

$$= -\frac{d}{dR} \cdot \frac{S}{R},$$

and similarly of the moon; and the equation of fluid equilibrium at the epoch gives

$$\frac{dp}{\rho'} = \frac{n^2}{2} d \cdot r'^2 \sin^2 \theta' + S d \cdot \frac{1}{R} + M d \cdot \frac{1}{R'},$$

r', θ', ϕ' being the coordinates to the particle of fluid. I shall at present leave out M (the moon), and, when the effect of S is found, add a similar term for M. Now

$$\frac{1}{R} = \frac{1}{c} + P_1 \frac{r'}{c^2} + P_2 \frac{r'^2}{c^3} + \dots,$$

$P_1, P_2, \dots$ being Laplace's coefficients. I shall integrate dp from the earth's centre, along r', to the surface of the crust, keeping θ' and ϕ' constant. Then

$$p = n^2 \left(\frac{2}{3} + \frac{1}{3} - \mu'^2 \right) \int_0^r \rho' r' dr' + \frac{S}{c^3} \left(P_1 \int_0^r \rho' dr' + 2 P_2 \int_0^r \rho' r' dr' + \right).$$

Substitute this in the formulæ of the last paragraph, observing that as ϵ^2 is to be neglected, the means a and a' may be put for r and r'. Observing the properties of Laplace's functions, and

putting

$$\int_0^a \rho' d \cdot a'^2 = f(a),$$

we have the value of L for fluid pressure

$$= a^3 \epsilon f(a) \int_{-1}^{1} \int_0^{2\pi} \left(n^2 \left(\frac{1}{3} - \mu'^2 \right) + \frac{2S}{c^3} P_2 \right) \mu' \sqrt{1 - \mu'^2} \sin \phi' d\mu' d\phi',$$

by integration and a property of Laplace's functions

$$= 2a^3 \epsilon f(a) \frac{S}{c^3} \cdot \frac{4\pi}{5} \mu \sqrt{1 - \mu^2} \sin \phi = \frac{8\pi a^3 \epsilon S}{5c^3} f(a) \cos\theta \sin\theta \sin\phi.$$

Similarly the value of the moment M for fluid pressure is

$$= - \frac{8\pi a^3 \epsilon S}{5c^3} f(a) \cos \theta \sin \theta \cos \phi.$$

There will be similar terms for the moon.

6. Hence the final values for the moments L and M are

$$\left(C - A + \frac{8\pi a^3 \epsilon}{15} f(a) \right) \left(\frac{3S}{c^3} \cos \theta \sin \theta \sin \phi + \frac{3M}{c_i^3} \cos\theta_i \sin\theta_i \sin\phi_i \right)$$

$$- \left(C - A + \frac{8\pi a^3 \epsilon}{15} f(a) \right) \left(\frac{3S}{c^3} \cos \theta \sin \theta \cos \phi \right) + \frac{3M}{c_i^3} \cos\theta_i \sin\theta_i \cos\phi_i \right).$$

Put

$$\left(1 + \frac{8\pi a^3 \epsilon f(a)}{15(C-A)} \right) S = S' \text{ and } \left(1 + \frac{8\pi a^3 \epsilon f(a)}{15(C-A)} \right) M = M',$$

then the differential equations become

$$A \frac{d\omega_1}{dt} + (C-A)n\omega_2 = (C-A) \left(\frac{3S'}{c_i^3} \cos \theta \sin \theta \sin \phi \right.$$
$$\left. + \frac{3M'}{c_i^3} \cos \theta_i \sin \theta_i \sin \phi_i \right),$$

$$A \frac{d\omega_2}{dt} - (C-A)n\omega_1 = -(C-A) \left(\frac{3S'}{c^3} \cos \theta \sin \theta \cos \phi \right.$$
$$\left. + \frac{3M'}{c_i^3} \cos \theta_i \sin \theta_i \cos \phi_i \right).$$

These equations are precisely the same as the ordinary equations for finding precession and nutation in a solid body, S' and M' being put for S and M. The solution, therefore, putting P for the precession of the crust, leads to

$$P = \frac{C-A}{C} (\alpha S' \perp \beta M') t*,$$

* Mechanical Philosophy, p. 437.

α and β being known quantities independent of the crust and fluid,

$$= \left(\frac{C-A}{C} + \frac{8\pi a^3 \epsilon f(a)}{15\,C}\right)(\alpha S + \beta M)t.$$

Let P, C, A be the values of P, C, A when the whole earth is solid. Then

$$P = \frac{C-A}{C}(\alpha S + \beta M)t;$$

$$\therefore \frac{P}{P} = \frac{C}{C-A}\left(\frac{C-A}{C} + \frac{8\pi a^3 \epsilon f(a)}{15\,C}\right).$$

If we substitute for A, C, A, C* and neglect the square of ellipticities, we have

$$\frac{P}{P} = \frac{\int_0^a \rho' \frac{d \cdot a'^5}{da'} da'}{\int_a^a \rho' \frac{d \cdot a'^5}{da'} da'} \cdot \frac{\int_a^a \rho' \frac{d \cdot a'^5 \epsilon'}{da'} da' + a^3 \epsilon \int_0^a \rho' \frac{d \cdot a'^2}{da'} da'}{\int_0^a \rho' \frac{d \cdot a'^5 \epsilon'}{da'} da'}.$$

Now evidently

$$a^3 \epsilon \int_0^a \rho' \frac{d \cdot a'^2}{da'} da' \text{ is greater than } \int_0^a \rho' \frac{d \cdot a'^5 \epsilon'}{da'} da',$$

since a and ϵ are greater than any other value of a' and ϵ'. Hence the second factor in the value of $P \div P$ is greater than unity; and the first factor is evidently greater than unity. Hence P is greater than P, or the crust has more precessional motion during the time my equations hold than the whole earth supposed solid. It follows, then, that even if at any instant the whole arrangement and motion of the crust and fluid nucleus are the same as if they were one solid (which really cannot have occurred), nevertheless that state could not continue; and it is a mistake to say that the motion of the crust will be the same whether the interior is fluid or solid.

7. It will be observed that the above formula of Precession does not coincide exactly with Mr. Hopkins's. For when ρ' is constant P does not $=$ P, as he says it does. The fact is, that my calculation above is purposely made on the hypothesis of M. Delaunay's view being true, and that, at least for a time, the instantaneous axis of rotation of the fluid coincides with the axis of the crust; and I show that the hypothesis immediately breaks down; whereas Mr. Hopkins assumes from the beginning the correct state of things, that the instantaneous axis of rotation of

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* Mechanical Philosophy, p. 534.

the fluid does not ever coincide exactly with the axis of the crust, and he obtains what amounts to the following formula,

$$\frac{P}{P} = \frac{\displaystyle\int_0^a \rho' \frac{d \cdot a'^5}{da'} da'}{\displaystyle\int_a^a \rho' \frac{d \cdot a'^5}{da'} da' + a^3 \int_0^a \rho' \frac{d \cdot a'^2}{da'} da'} \cdot \frac{\displaystyle\int_a^a \rho' \frac{da'^5 \epsilon'}{da'} da' + a^3 \epsilon \int_0^a \rho' \frac{da'^2}{da'} da'}{\displaystyle\int_0^a \rho' \frac{d \cdot a'^5 \epsilon'}{da'} da'},$$

which differs from mine by one term in the first denominator. This, it will be seen, equals unity when ρ' is constant.

I am,

Yours faithfully,

JOHN H. PRATT.

Calcutta, June 9, 1871.

XIII. *On a Practical Method for detecting bad Insulators on Telegraph Lines.* By LOUIS SCHWENDLER, *Esq.**

ONE of the many practical measures, and certainly not one of the least important, introduced during the last few years with the view of increasing the efficiency of the telegraph department, is the establishment of a scientific system of testing all materials and instruments employed on the line. Many practical results have already been obtained therefrom; but it is not the object of the present communication to enter into the details of this most interesting subject; I will only point out one important fact that has been established :—

A great many lines in India contain electrically defective insulators—some to such an extent as to lower the insulation to a degree which is fatal to the direct and regular working of long lines.

How such insulators could creep in, notwithstanding the care taken in England to secure efficient telegraph stores for India, is a question with which I cannot deal at present, but which may perhaps form the subject of a *future* paper when more data have been collected†.

The very fact that electrically defective insulators, showing nothing externally, do exist and are distributed over lines of such

* From the Proceedings of the Asiatic Society of Bengal for March 1871. Communicated by the Author.

† The cause for the low insulation of insulators seems to be the porous state of some porcelain, through which a minute quantity of water diffuses itself in time. When an imperfect insulator is heated, it always becomes perfect; but immersed a sufficiently long time in water it becomes again imperfect.

The leakage seems to be invariably in that part of a porcelain which is cemented in the iron hood.

vast extent, has created the necessity of having a reliable method
by which such insulators can be detected, and other perfect ones
substituted with the least possible expense.

It is clear that such a method, to be practicable, must be very
simple, and the instruments used portable and handy*.

After some searching in this direction, the following method
was found to answer the purpose most satisfactorily.

The principle of the method is to produce magneto-electric
currents through the resistance of the insulator under test, and
to measure these currents by the effect they have on the body of
the tester.

The subjoined diagram shows the connexions readily.
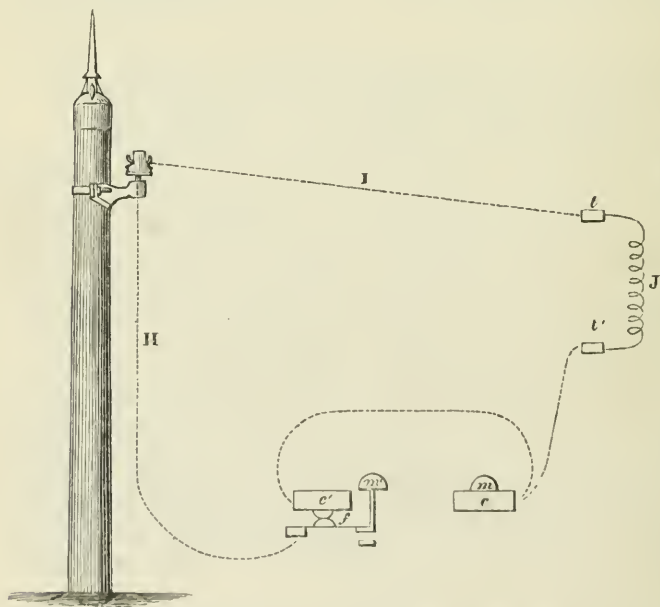
J is a magneto-electric machine, the two terminals t and t' of
which are insulated from each other and from the ground.

t is in permanent contact with a perfectly insulated leading
wire I, long enough to reach the insulator, to the iron hood of
which it is to be hooked.

* To use a deflection method is out of the question, because the still
comparatively high resistance of the insulators which have to be detected
would necessitate a high electromotive force and a very delicate galvano-
meter, which arrangements could not be made easily portable, as is required
when the tester proceeds along a line.

t' is in permanent connexion with the clamp c, to which is fixed a small platinum knob m; and both the clamps c and c' are permanently connected with each other. A well-insulated lead-ing wire, II, which is to be hooked on to the bracket of the insu-lator under test, is in contact with the moveable platinum knob m', which, however, is insulated from c' when pressed down, but which in its position of rest (or when not pressed down short) closes the circuit between c and c' at f.

The whole arrangement is constructed light but strong, pro-tected from rain, and can be carried along the line by one man only.

The tester proceeds as follows :—

After having cleaned the insulator carefully, he removes tem-porarily the line from the insulator and hooks the leading wire No. I to the iron hood, and leading wire No. II to the bracket of the insulator. He then turns the handle of the magneto-electric machine with one hand, while one finger of the other is resting on the knob m of clamp c.

As soon as he touches with the other finger the knob m' of clamp c', at the same time pressing it down, the metallic circuit between c and m' is opened, and the positive and negative mag-neto-electric currents have to pass from one finger to the other, and consequently, if strong enough, will give the tester sensible shocks, by which he is at once informed that the insulator under test is defective, and much under the fixed standard of insulation.

If the tester does not feel any current through his fingers (a comparatively rough galvanoscope), he has only to repeat the experiment by placing his tongue on the knob m while his hand still presses the knob m' down. If no current is felt by the tester through this most delicate galvanoscope (the tongue), he can rest assured that the insulator is perfect for all practical purposes.

By opening and closing the circuit alternately at the knob m' the tester has it in his power to allow at short intervals *currents* to pass through his tongue, and consequently will be able to de-tect the slightest induction-currents.

The following experiments were made with insulators of known resistance, to ascertain the highest limit by which the tongue is still able to detect induction-currents.

The currents in these experiments were produced by one of Siemens's well-known dial instruments, the revolving bobbin of which had a resistance $= 1577$ S. units.

The absolute resistance of each insulator was first carefully measured in the ordinary manner without water in the porcelain cups, and the insulator afterwards tested by the method above described.

No. of insulator.	Resistance in *millims.* S. units.	Strength of magneto-electric currents (as indicated by the human body) through the resistance of the insulator under test.
1.	0·11	Strong shocks felt by fingers.
2.	0·13	do. do.
3.	0·145	do. do.
4.	0·19	do. do.
5.	0·75	Slight shocks felt by fingers.
6.	2·30	{ No shocks felt by fingers, but good shocks through tongue.
7.	5·70	{ No shocks felt by tongue, but a strong acid taste.
8.	7·1	Distinct but slight acid taste
9.	8·2	do. do.
10.	82·0	Nothing felt by tongue.
11.	189·0	do. do.
12.	615·0	do. do.
13.	2520·0	do. do.
14.	∞	do. do.

From these experiments it follows that all insulators offering a resistance up to about 1 millim. S. unit can be detected by the fingers, and those above 1 millim. and under 8 millims. can be unmistakably detected by the tongue. It appeared also that tongues of different persons were equally sensitive, since several persons (Europeans and natives) acknowledged the known acid taste, even through the insulator No. 9, having 8·2 millims. S. units resistance.

The highest limit of the method could, of course, be increased by filling the revolving bobbin of the magneto-electric machine with much finer wire and increasing the number of permanent magnets; however this will be scarcely necessary, because it seems to be a fact that if an insulator has more than about 8 millims., the resistance is generally so high as to be practically infinite, and therefore a greater sensitiveness of the instrument would only complicate the method.

As it is intended that the tester himself should turn the handle of the magneto-electric machine, he has it entirely in his power to regulate the strength of the induction-currents by turning faster or slower; and as, besides this, he always begins the testing by at first sending the currents through his fingers, no severe shocks can occur to him in the subsequent operation.

The method has also a safeguard in itself against carelessly rejecting good insulators, because the tester will certainly be careful in having the insulator properly cleaned before testing it, in order to avoid severe shocks.

There can also be scarcely any doubt that the tongue is the

best detector in this particular case, because it is sufficiently sensitive, never gets out of order, and indicates almost momentary currents; it is besides the cheapest instrument that could be used.

Note.—This method may also with advantage be used for detecting bad joints in a telegraph line. It is then only necessary to connect the two ends of the joint to the two terminals of the magneto-electric machine in such a way that the body of the tester acts as a shunt to the joint.

A joint which offers a resistance of not less than 5 S. units allows a current to pass sufficiently strong to be detected by the tongue; but if the joint has a resistance of more than 200 S. units, the current passing is strong enough to be felt already by the fingers of the tester.

XIV. *Investigation of the Law of the Progress of Accuracy, in the usual process for forming a Plane Surface. By* GEORGE BIDDELL AIRY, *Astronomer Royal*[*].

IN order to form a plane surface, it is usual to take three surfaces (which I shall call A, B, C), and to grind A with B till they fit together, then to grind B with C, then to grind C with A. (I shall call each of these grindings a *rub*, and the system of three rubs an *operation*.) And the problem which I propose is, to find the deviation of each surface from a plane, after n operations, expressed as a function of n.

I shall assume that at each rub the surfaces are worked into perfect contact. Putting A and B for the prominences of special parts of the surfaces of A and B above a mean plane, and putting A′ and B′ for the state to which they are changed after the rub, and remarking that convexity of one corresponds to concavity of the other, A′ + B′ must = 0. I shall also assume that equal portions are worked off the two surfaces—that is, that A − A′ = B − B′. These equations give

$$A' = \tfrac{1}{2}A - \tfrac{1}{2}B, \quad B' = -\tfrac{1}{2}A + \tfrac{1}{2}B.$$

Considering now the effect produced by the first of all the operations, the expressions for the prominences are as follows:—

	A	B	C
Before rubbing	A	B	C
After first rub	$+\tfrac{1}{2}A - \tfrac{1}{2}B$	$-\tfrac{1}{2}A + \tfrac{1}{2}B$	C
After second rub.	$+\tfrac{1}{2}A - \tfrac{1}{2}B$	$-\tfrac{1}{4}A + \tfrac{1}{4}B - \tfrac{1}{2}C$	$+\tfrac{1}{4}A - \tfrac{1}{4}B + \tfrac{1}{2}C$
After third rub, completing the first operation.	$+\tfrac{1}{8}A - \tfrac{1}{8}B - \tfrac{1}{4}C$	$-\tfrac{1}{4}A + \tfrac{1}{4}B - \tfrac{1}{2}C$	$-\tfrac{1}{8}A + \tfrac{1}{8}B + \tfrac{1}{4}C$

I shall not delay longer on this first operation, except to make the following remark. At the end of this first operation, the new values of A and C are equal but with opposite signs; and it is evident that the same remark will apply to the result of every succeeding operation. But it did not apply to the values A, C with which we started. We must therefore consider these values obtained after the first operation as the beginning of the symbolical series; and we may call them A_0, B_0, $-A_0$.

Suppose now that we have gone through n operations, forming the values A_n, B_n, $-A_n$; and that we examine the effect of the $n+1$th operation. We have

	A_n	B_n	$-A_n$
Before rubbing .	A_n	B_n	$-A_n$
After first rub .	$+\frac{1}{2}A_n - \frac{1}{2}B_n$	$-\frac{1}{2}A_n + \frac{1}{2}B_n$	$-A_n$
After second rub.	$+\frac{1}{2}A_n - \frac{1}{2}B_n$	$+\frac{1}{4}A_n + \frac{1}{4}B_n$	$-\frac{1}{4}A_n - \frac{1}{4}B_n$
After third rub, completing the $n+1$th operation	$+\frac{3}{8}A_n - \frac{1}{8}B_n$	$+\frac{1}{4}A_n + \frac{1}{4}B_n$	$-\frac{3}{8}A_n + \frac{1}{8}B_n$
Which are the same as .	A_{n+1}	B_{n+1}	$-A_{n+1}$

We have therefore the equations

$$A_{n+1} = +\frac{3}{8}A_n - \frac{1}{8}B_n,$$

$$B_{n+1} = +\frac{1}{4}A_n + \frac{1}{4}B_n.$$

Multiply the second by an indeterminate constant p, and add it to the first; then

$$(A_{n+1} + p \cdot B_{n+1}) = \left(\frac{3}{8} + \frac{1}{4}p\right)A_n + \left(-\frac{1}{8} + \frac{1}{4}p\right)B_n,$$
$$= \left(\frac{3}{8} + \frac{1}{4}p\right) \cdot \left\{A_n + \frac{-1 + 2p}{3 + 2p}B_n\right\}.$$

Determine p so that $\dfrac{-1 + 2p}{3 + 2p} = p$; and put q for the corresponding value of $\dfrac{3}{8} + \dfrac{1}{4}p$. Then the equation is

$$(A_{n+1} + p \cdot B_{n+1}) = q \cdot (A_n + p \cdot B_n),$$

of which the solution is

$$A_n + p \cdot B_n = E \cdot q^{n+\beta};$$

where E is a constant, to be determined so as to satisfy initial circumstances, and where β may be fractional.

As the equation for p will be a quadratic, there will be two

values of p, two corresponding values of q, and two permissible values of E and of β; and the solutions will be

$$A_n + p' \cdot B_n = E' \cdot (q')^{n+\beta'},$$
$$A_n + p'' \cdot B_n = E'' \cdot (q'')^{n+\beta''};$$

from which A_n and B_n may be found.

Forming the equation for p, we find $2p^2 + p + 1 = 0$; the roots of which may be expressed in the form

$$p' = -\sqrt{\tfrac{1}{2}} \cdot \left\{ \sqrt{\tfrac{1}{8}} - \sqrt{-1} \cdot \sqrt{\tfrac{7}{8}} \right\},$$
$$p'' = -\sqrt{\tfrac{1}{2}} \cdot \left\{ \sqrt{\tfrac{1}{8}} + \sqrt{-1} \cdot \sqrt{\tfrac{7}{8}} \right\};$$

and substituting in the formula $\tfrac{3}{8} + \tfrac{1}{4} p$,

$$q' = -\sqrt{\tfrac{1}{8}} \cdot \left\{ -\sqrt{\tfrac{25}{32}} - \sqrt{-1} \cdot \sqrt{\tfrac{7}{32}} \right\},$$
$$q'' = -\sqrt{\tfrac{1}{8}} \cdot \left\{ -\sqrt{\tfrac{25}{32}} + \sqrt{-1} \cdot \sqrt{\tfrac{7}{32}} \right\}.$$

Let

$$\cos \alpha = \sqrt{\tfrac{1}{8}}, \quad \sin \alpha = \sqrt{\tfrac{7}{8}}, \quad (\alpha = 69^\circ \ 17' \ 43'' \text{ nearly});$$

then it will be found that

$$-\sqrt{\tfrac{25}{32}} = \cos 3\alpha, \quad -\sqrt{\tfrac{7}{32}} = \sin 3\alpha;$$

and the expressions become

$$p' = -\sqrt{\tfrac{1}{2}} \cdot (\cos \alpha \ - \sqrt{-1} \cdot \sin \alpha),$$
$$q' = -\sqrt{\tfrac{1}{8}} \cdot (\cos 3\alpha + \sqrt{-1} \cdot \sin 3\alpha),$$
$$p'' = -\sqrt{\tfrac{1}{2}} \cdot (\cos \alpha \ + \sqrt{-1} \cdot \sin \alpha),$$
$$q'' = -\sqrt{\tfrac{1}{8}} \cdot (\cos 3\alpha - \sqrt{-1} \cdot \sin 3\alpha).$$

It appears here that $(p')^3 = q''$, and $(p'')^3 = q'$; and this is verified in the following manner. From the equation for p just employed,

$$0 = p^3 + \tfrac{1}{2} p^2 + \tfrac{1}{2} p,$$
$$0 = \quad -\tfrac{1}{2} p^2 - \tfrac{1}{4} p - \tfrac{1}{4}.$$

The sum is

$$0 = p^3 + \tfrac{1}{4} p - \tfrac{1}{4}, \text{ or } p^3 = \tfrac{1}{4} - \tfrac{1}{4} p.$$

Therefore $(p')^3 = \tfrac{1}{4} - \tfrac{1}{4} p'$. But, in the second term of the

equation,
$$p' + p'' = -\frac{1}{2}, \quad p' = -\frac{1}{2} - p'', \quad \frac{1}{4} - \frac{1}{4}p' = \frac{3}{8} + \frac{1}{4}p'' = q'',$$
and therefore $(p')^3 = q''$. Similarly $(p'')^3 = q'$.

The expressions for p and q may be more conveniently put in the following form :—

$$p' = \sqrt{\tfrac{1}{2}} \cdot \{\cos \overline{\alpha + \pi} \quad - \sqrt{-1} \cdot \sin \overline{\alpha + \pi}\},$$

$$q' = \sqrt{\tfrac{1}{8}} \cdot \{\cos \overline{3\alpha + 3\pi} + \sqrt{-1} \cdot \sin \overline{3\alpha + 3\pi}\},$$

$$p'' = \sqrt{\tfrac{1}{2}} \cdot \{\cos \overline{\alpha + \pi} \quad + \sqrt{-1} \cdot \sin \overline{\alpha + \pi}\},$$

$$q'' = \sqrt{\tfrac{1}{8}} \cdot \{\cos \overline{3\alpha + 3\pi} - \sqrt{-1} \cdot \sin \overline{3\alpha + 3\pi}\};$$

and

$$(q')^{n+\beta'} = \left(\tfrac{1}{8}\right)^{\frac{n}{2}+\frac{\beta'}{2}} \cdot \{\cos \overline{(n+\beta' \cdot 3\alpha + 3\pi)} + \sqrt{-1} \cdot \sin \overline{(n+\beta' \cdot 3\alpha + 3\pi)}\},$$

$$(q'')^{n+\beta''} = \left(\tfrac{1}{8}\right)^{\frac{n}{2}+\frac{\beta''}{2}} \cdot \{\cos \overline{(n+\beta'' \cdot 3\alpha + 3\pi)} + \sqrt{-1} \cdot \sin \overline{(n+\beta'' \cdot 3\alpha + 3\pi)}\}.$$

In algebraic generality there is nothing to prevent E from consisting of two terms, one being imaginary. But such an expression could be put under the form of a cosine and a sine with imaginary factor, and its effect would be simply to add a constant to the constant β, and nothing would really be gained in generality. And, upon attempting to solve the equations for A_n and B_n, it would be found immediately that the condition of real values for A_n and B_n requires that E' and E'' be equal, and that β' and β'' be equal. The equations are therefore to be used in this form :—

$$A_n + p' \cdot B_n = E \cdot (q')^{n+\beta},$$
$$A_n + p'' \cdot B_n = E \cdot (q'')^{n+\beta}.$$

First, taking their difference, we find

$$B_n = E \cdot \frac{(q')^{n+\beta} - (q'')^{n+\beta}}{p' - p''}$$

$$= -E \cdot \left(\tfrac{1}{8}\right)^{\frac{n}{2}+\frac{\beta}{2}} \cdot \frac{2\sqrt{-1} \cdot \sin \overline{(n+\beta \cdot 3\alpha + 3\pi)}}{2\sqrt{\tfrac{1}{2}} \cdot \sqrt{-1} \cdot \sin \overline{(\alpha + \pi)}}$$

$$= -E \cdot \frac{\sqrt{2}}{\sin \overline{(\alpha + \pi)}} \cdot \left(\tfrac{1}{8}\right)^{\frac{n}{2}+\frac{\beta}{2}} \cdot \sin \overline{(n+\beta \cdot 3\alpha + 3\pi)}.$$

Second, multiplying the first equation by $p'' = (q')^{\frac{1}{3}}$ and the

second by $p' = (q'')^{\frac{1}{3}}$,

$$p'' \cdot A_n + p' \cdot p'' \cdot B_n = E \cdot (q')^{n+\frac{1}{3}+\beta},$$

$$p' \cdot A_n + p' \cdot p'' \cdot B_n = E \cdot (q'')^{n+\frac{1}{3}+\beta};$$

and subtracting the upper from the lower,

$$(p' - p'') \cdot A_n = E \cdot \{ -(q')^{n+\frac{1}{3}+\beta} + (q'')^{n+\frac{1}{3}+\beta} \};$$

or

$$-2\sqrt{\tfrac{1}{2}} \cdot \sqrt{-1} \cdot \sin(\alpha + \pi) \cdot A_n$$

$$= -2E \cdot \left(\tfrac{1}{8}\right)^{\frac{n}{2}+\frac{1}{6}+\frac{\beta}{2}} \cdot \sqrt{-1} \cdot \sin\overline{\left(n + \tfrac{1}{3} + \beta \cdot \overline{3\alpha + 3\pi}\right)},$$

from which

$$A_n = +E \cdot \frac{\sqrt{2}}{\sin(\alpha + \pi)} \cdot \left(\tfrac{1}{8}\right)^{\frac{n}{2}+\frac{1}{6}+\frac{\beta}{2}} \cdot \sin\overline{\left(n + \tfrac{1}{3} + \beta \cdot \overline{3\alpha + 3\pi}\right)}.$$

The complicated constant $E \cdot \dfrac{\sqrt{2}}{\sin(\alpha + \pi)}$ may be expressed as a single constant F, and then we have

$$A_n = +F \cdot \left(\tfrac{1}{8}\right)^{\frac{n}{2}+\frac{1}{6}+\frac{\beta}{2}} \cdot \sin\overline{\left(n + \tfrac{1}{3} + \beta \cdot \overline{3\alpha + 3\pi}\right)},$$

$$B_n = -F \cdot \left(\tfrac{1}{8}\right)^{\frac{n}{2}+\frac{\beta}{2}} \cdot \sin\overline{(n + \beta \cdot \overline{3\alpha + 3\pi})}.$$

The constants F and β are to be determined by making

$$A_0 = +F \cdot \left(\tfrac{1}{8}\right)^{\frac{1}{6}+\frac{\beta}{2}} \cdot \sin\overline{\left(\tfrac{1}{3} + \beta \cdot \overline{3\alpha + 3\pi}\right)},$$

$$B_0 = -F \cdot \left(\tfrac{1}{8}\right)^{\frac{\beta}{2}} \cdot \sin(\beta \cdot \overline{3\alpha + 3\pi}).$$

It has been found by actual substitution that the values found for A_n and B_n satisfy the original equations. I consider this proof of correctness to be necessary when real values are inferred from a process conducted by means of imaginary quantities.

It is worthy of remark that the expression for the amount of prominence consists, in each case, of the product of two terms which vary with the number of operations. The second term is periodical, showing that the prominence may even change sign. But the first shows a rapid decrease in geometrical progression: one operation makes a reduction in the proportion of $14 : 5$ nearly; two operations, in the proportion of $8 : 1$; four operations, in the proportion of $64 : 1$, &c. The rapid approach to a truly plane surface is thus explained.

Royal Observatory, Greenwich,
July 11, 1871.

XV. *On Statical and Dynamical Ideas in Chemistry.*—Part. III. *The Atomic Theory.* By EDMUND J. MILLS, *D.Sc.**

CONTENTS.

A CRITICAL examination of some of the leading ideas rela-ting to Chemical Functions and Chemical Substance†, conducts, by an easy and simple transition, to a discussion of the Atomic Theory. If the reader will admit as sound the cha-racteristic of a universal criterion pointed out in Part I., he may now feel an interest in reading the oldest legend of systematic physics by the light of the latest and best development of modern science. The idea of motion, which is the criterion in question, comes inevitably to be accepted as the sole reliable guide, when a guide is sought in scientific controversy : it does not appeal to nor is it derived from this or that authority ; but, as the common property of every one who reflects, and drives his conclusions to their end, it is immediately and independently available.

The primary postulate of the atomic theory is the existence of indivisibles. From this demand it has never receded, whatever may have been the state of contemporary science. From Lucre-tius, whose lines

> Hæc neque dissolvi plagis extrinsecus icta
> Possunt ; nec porro penitus penetrata retexi ;
> Nec ratione queunt alia tentata labare. [I. 531–533.]

> [Nor, struck with outer blows, can these dissolve ;
> Nor, penetrated deep, be disentwined ;
> Nor, tried in other mode, can waver aught.]

are explicit upon this point, to the majority of modern chemical writers, this supposition pursues an undeviating course. Upon the different properties of these indivisibles, however, all their

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* Communicated by the Author.
† Phil. Mag. S. 4. vol. xl. p. 259.

defenders are not agreed.   Dalton, for example (New System, pp. 135, 136), conceives water to have an internal constitution resembling square piles of shot arranged in successive horizontal strata.   " A vessel full of any pure elastic fluid " also " presents to the imagination a picture like one full of small shot " (pp. 147 & 189).   When two gases are mixed, an intestine motion ensues, and continues " till the particles arrive at the opposite surface of the vessel against any point of which they can rest with stability, and the equilibrium at length is acquired when each gas is uni- formly diffused through the other " (p. 190).   Solids likewise consist of arranged particles (p. 209).   Hence it is evident that Dalton regarded atoms as enjoying a perfect repose, unless when mechanically or chemically disturbed.   In this purely statical contemplation he has been followed by most chemists, some of whom have believed themselves to dissent wholly from his theory. What is the theory of types, but an emanation from the prime idea involved in the celebrated figures at the end of the ' New System '?   The wooden models with which Dalton illustrated his lectures have reappeared as glyptic formulæ; and the material existence of their connecting wires is perpetuated in the lines or " bonds " of graphic formulæ.

But atoms have been considered from another point of view. It has been found by not a few thinkers that rest is a condition which falsely represents the facts of nature, and that atoms must therefore be conceived as moving with an industry to which cessa- tion is unknown.   On this view, the state of dissolved salts and the process of precipitation are explained much as Berthollet ex- plained them, only in corpuscular language.   Supposing, for example, hydric chloride be added to aqueous cupric sulphate. That cupric chloride is formed is shown by the green coloration that ensues.   Hence there has been a partition of the copper.   This is accounted for by supposing all the atoms in the mixture to move constantly—by adopting, in short, the theory of greatest effort.   Another chemist observes that if all the atoms were per- fectly free to move, no compound could be stable, and conse- quently brings forward an hypothesis of "limited atomic mobility."

Considerations derived from chemical formulæ have frequently been adduced in favour of the atomic theory, and therefore de- serve attention.   The formulæ themselves were at first the results of experimental facts in quantitative analysis, and are therefore independent of theory.   Dalton first applied a method of sym- bolic ratios, in which attention was especially drawn, first, to a standard unit for each element, and secondly to that unit's co- efficient.   The standard unit is then taken as the atom; a ne- cessary consequence of which supposition is, that all coefficients must be integral.   It will be observed, however, that we are now

no longer dealing with fact but with a crude conjecture; and the well-known rules given by Dalton for ascertaining the complexity of a combination, and intended to develope his system of ratios, are more conjectural still :—If only one combination of two elements exist, it must be presumed to be binary; when three combinations are obtained, we may expect one to be a binary, and the other two ternary, and so on[*]. Berzelius himself felt enamoured of the theory, and occasionally turned aside from the path of induction to guesswork of a similar kind :—Strong bases consist of a radical taken once, combined with oxygen taken once[†]; and, "if a combustible radical[‡] unite with oxygen in several proportions, these are compared, and the result is reduced to the smallest number of atoms possible." With the introduction of the word *equivalent* by Wollaston, the symbolic unit acquired a new implication; and the historical reader is familiar with the confusion produced and maintained for very many years by this insertion of a peculiarly dynamical idea in the ungenial soil of contemporary speculation. Writers of text-books failed to understand their position ; and the science was still struggling underground when the splendid discovery of Dulong and Petit gave it vigour and foliage. This discovery, indeed, was left to other hands to improve or perfect; but by the distinct relation it evinced between the symbolic standard and specific heat, decision and certainty were able to displace conjecture. Another important step was to adopt a uniform understanding as to the relation of composition to specific gravity of vapour, this understanding having been based upon formulæ which had been determined by the previously existing canons. The indications afforded by isomorphism are of minor value. But throughout these various processes—establishing by analytical comparison certain standard units and ratios, discovering that certain of these were connected with the specific heat by a simple law, that to these a uniform vapour-volume may be assigned, and that similarity of chemical function frequently involves similarity of form (another function)—where does indivisibility suggest itself? Still less is the existence of an atom *proved*.

I now pass from the singular relations of formulæ to consider them as existing in equations. Let it be required, for instance, to find the formula of benzol. The values $C = 12$, $H = 1$ are given. The analytical result, in terms of these values, is C H. Recourse is then had to chlorination, which occurs, or may be taken to occur, in successive stages ; the product at each stage is analyzed ($Cl = 35·5$), and the corresponding minimum equations are written thus :—

    * New System, p. 214.
    † *Traité de Chimie* [Esslinger], vol. iv. p. 604.
    ‡ *Théorie des Proportions Chimiques* (1819), p. 117.

Resulting symbols.   Generating symbols.

$$C^6 H^5 Cl + HCl = C^6 H^6 + Cl^2,$$
$$C^3 H^2 Cl + HCl = C^3 H^3 + Cl^2,$$
$$C^2 H \ Cl + HCl = C^2 H^2 + Cl^2,$$
$$C^3 H \ Cl^2 + 2HCl = C^3 H^3 + 2Cl^2,$$
$$C^6 H \ Cl^5 + 5HCl = C^6 H^6 + 5Cl^2,$$
$$C \quad Cl + HCl = C \ H + Cl^2.$$
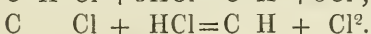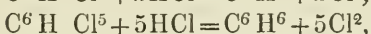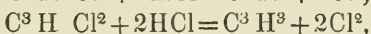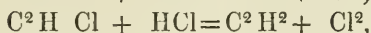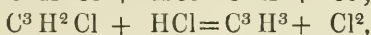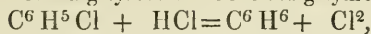
The passage from chlorine derivative to parent compound is effected by the numerical exchange of Cl for H, the validity of which need not be disputed, though it is often much misunderstood. We then have four formulæ—$C^6 H^6$, $C^3 H^3$, $C^2 H^2$, C H —between which to choose. Now the equational method, if I may so term it, does not exactly choose between these, but *takes the least common multiple of them,* and, *on that ground,* decides on $C^6 H^6$ as the formula of benzol. The doctrine of replacement in successive stages (as, for instance, the treble ethylation of ammonia) is substantially identical in its symbolic exposition with the equational method. Now this method is held to point out that the " atom of benzol weighs $C^6 H^6$." Is it necessary to indicate that the use of the process for finding the least common multiple is not atomic, and that, if so, arithmetic should also possess its atomic theory?

If the prejudices of education were less complete or less closely intertwined than they are, I might hopefully ask atomic theorists whether, as professedly inductive reasoners, they are still pleased to accept these shadows for reality. My argument may have a better fortune, perhaps, with those who sit loose to theory, or at least will not voluntarily put on the shirt of Nessus. Accordingly, it may be advantageous if, after proceeding from the construction of symbols to equations, we visit the atomic theory in a spot that is peculiarly its home, namely the province of isomerism. It is here that, we are told, the greatest victories of that theory have been won, that it is of the highest practical utility, and where, without it, we can conceive no other guide to an explanation[*]. The following special case, which, I believe, has no exceptional features, is selected from Kekulé's *Lehrbuch* (vol. ii. pp. 257, 258), and relates to the well-known instances of the two isomers $C^4 H^4 O^4$, and the three isomers $C^5 H^6 O^4$. These bodies, which are known as fumaric and maleic acid, and itaconic, citraconic, and mesaconic acid, respectively, unite each with the same quantity, of hydrogen to form succinic acid, $C^4 H^6 O^4$, and pyrotartaric acid, $C^5 H^8 O^4$. Each of the isomers combines also with the same weight of bromine; but, contrary to expectation (based on the hydrogenating experiments), two dibromosuccinic acids,

[*] British Association Report for 1870 : Transactions of Sections, p. 45.

$C^4 H^4 Br^2 O^4$, and three dibromo-pyrotartrates ($C^5 H^6 Br^2 O^4$) are formed. We have therefore to explain how it is that these facts, assuming them to be true, are compatible with each other. Kekulé proceeds as follows :—

" In accordance with the views relating to the atomicity of the elements which we have previously unfolded on several occasions (see especially § 1369), all the affinities of the atoms comprising the molecule of succinic (or its homologue pyrotartaric) acid are saturated ; these acids constitute, in a manner, closed molecules. They contain two atoms of typical oxygen, that is, oxygen bound on to carbon by only one of its two units of affinity. Two hydrogen atoms are united with the carbon only through the intervention of these typical oxygen atoms. These two typical hydrogen atoms are easily displaceable by metals ; but there are two other oxygen atoms present which are attached to the carbon by two units of affinity, and consequently, in the language of the type theory, belong to the radical.

" Now, it is easy to see that, in addition to these two typical hydrogen atoms, succinic has four, and pyrotartaric acid six, more atoms of hydrogen. This hydrogen, which in the language of type theory belongs to the radical, is, according to the theory of the atomicity of the elements, directly combined with the carbon—in fact, in such a manner that two hydrogen atoms are always united with the same carbon atom.

" Next, let us assume that two such hydrogen atoms are absent (*fehlen*) from one or other of these two normal acids ; we have, on the one hand, the composition of fumaric and maleic acid, the formulæ of itaconic, citraconic, and mesaconic acid on the other. But since in succinic acid two pairs of such hydrogen atoms are bound to the carbon, two acids may evidently exist with less hydrogen ; and similarly, in the case of pyrotartaric acid, we can understand how there may be three isomers with less hydrogen, according to the absence of one or other of the three pairs of hydrogen atoms, which in the normal substance are directly united with the carbon.

" At that place in the molecule whence the two hydrogen atoms are absent, two units of affinity of the carbon are left unsaturated ; at that place there is, so to speak, a gap. Hence we can explain the exceptional facility with which these substances unite, by way of addition, with hydrogen or bromine. The free units of affinity of the carbon make an effort to saturate themselves, and so to fill up the gap.

" If hydrogen be introduced into these unoccupied spaces, all carbon atoms within the molecule are united to the same element—hydrogen ; there appears no ground for the existence of differently modified normal substances as so obtained. In

fact, only *one* succinic and *one* pyrotartaric acid are at present known.

"If, on the other hand, bromine be put in those same unoccupied spaces, the carbon within the molecule is partly united with hydrogen, partly with bromine; and it is readily perceived that different modifications of such bromo-acids must necessarily exist, according as the bromine finds itself in one or the other of those spaces."

This mode of explanation is now virtually common to all atomic theorists. It is assumed that substances consist of molecules, these again of atoms; that determinate space or position (*Stelle*) is conceivable without atoms, and exists indeed in their absence; and that into this space there stretch at all times mysterious units of affinity, which, when the atoms are no longer present, strive after combination. But if all chemical substances consist of atoms, and position is possible without them, how can such position be known or determined? According to Hegel, pure being is pure nothing; and we may assign pure position to the same category. But determinate space or position, with the only thing that can determine it taken away, is contradiction itself. Still more unsatisfactory are the units of affinity (*Verwandtchaftseinheiten*). They cannot be chemical substance, or they would be identical with the atom; they cannot be dynamical units, similar to foot-pounds, for no such integral relations as they would then present have been found in the measurements of chemical action. Yet unquestionably dynamical language is used of them; they are said "to make an effort," "to bind on," "to rivet together," &c. It is much to be deplored that no atomic theorist has yet thrown light on the obscure question of units of affinity, or even stated in clear terms what he means by them. In the absence of any such statement, I shall class them, as pure number, in the roomy category of Hegel.

If any one were to observe under a microscope a small insect with cephalic, thoracic, and abdominal appendages, and were afterwards to assert that these were six in number, there being two of each name—that he had removed two of them, which the insect made a proportionate effort to reunite to its body, and that as the remaining four were removed an increased struggle was manifest—such statements, I say, would be conceivably true. But when language like this is used to explain to me the "structure" of a succinate, I decline to accept either its substance or its form, until the facts alleged of the succinate are put upon the same footing as those asserted of the insect. Until then, "units of affinity" may be considered as false an expression as "units of hunger" would be now.

Kekulé himself, by admitting a class of isomers "im engeren

Sinne," has tacitly shown the weakness of the atomic interpretation of isomerism.

Those who consider isomerism explained by such methods as have just been discussed, must understand by an *explanation* something different from the scientific meaning of that term. A phenomenon is explained when it is shown to be a part or instance of one or more known and more general phenomena. Isomerism is not, therefore, explained by assertions about indivisibles, which have neither been themselves discovered nor shown to have any analogy in the facts or course of nature—nor by explicit statements about a "structure" which has never been seen—nor by the use of a phrase to which no clear definition has been, or can be, attached.

Before isomerism had acquired the importance it now possesses, the great argument in favour of the atomic theory was that the law of multiple and that of definite proportions undeniably represent facts which can be explained only by the existence of atoms. Here the inadequate and idle notion of an "explanation" recurs. I have already (p. 115) exposed the fallacy of supposing that the equational method of arriving at formulæ is any thing more than the arithmetical process of taking the least common multiple—and pointed out the imaginary nature of Dalton's rules (p. 114); and formulæ that have been obtained by these means, in order to affirm the law of multiple or definite proportions, are condemned accordingly. But the exact point of this argument, so far as it has not been alluded to already, lies in the following considerations. Supposing an aqueous solution of hydric chloride be mixed with successive small quantities of sodic hydrate. Several actions occur; but consider for a moment only that one whereby the hydrogen of the hydric chloride is exchanged for sodium. It is quite evident that, on each addition of the sodic hydrate, a new compound ought to be produced containing, say, the whole of the sodium, in the form $H_x Na_y Cl_z$; and as the quantity of the sodium may be varied infinitely at pleasure, an infinite variety of hydrosodic chlorides must, if matter be infinitely divisible, be the result of the process. But not only does such a variety not arise, there is a perfect absence of any hydrosodic compound; for the sole product of the reaction in every case is sodic chloride, Na Cl. Hence it is inferred that matter cannot be infinitely divisible— that there has been a *saltus*—in short, that atoms exist.

Now this is a question of a constant ratio considered as existing between sodium and chlorine when brought together under conditions which need not be constant. As the mixture is made, the sodium and chlorine are unquestionably divided by being dissolved in a larger quantity of liquid than before. If

10 grammes of sodium were originally present, they now consist of two lots of, say, 5 grammes each. This makes it quite conceivable that, without the addition of any more of the solvent, the division may continue of its own accord, say, into ten lots of 1 gramme each, a thousand lots of a decigramme each, and so on, without limit. The same is true of the chlorine. On endeavouring to prepare a compound from the solution, only one is obtained with an invariable ratio between the sodium and the chlorine. The reason of this phenomenon is presumed to be unknown and to be now sought. I can only say that the fact, from the above point of view, is as conceivable on the supposition of continuous as it is upon that of limited division. Two phenomena continuously proceeding without obvious end (mathematical " infinites ") are well known to be capable of a finite ratio. Through one point an infinite number of curves may be drawn. The neutralization of aqueous hydric chloride is something like the process of differentiation, and its result, a constant finite ratio, like a differential coefficient. Or take as an illustration the properties of the machine known as the " geometric chuck." By a suitable combination of circular movements, this beautiful instrument is capable of describing an endless variety of curves, one of which is roughly represented in the margin : such a figure is drawn by a motion which is visibly continuous, even at the three points, throughout the entire period of delineation ; when it has been described, the machine proceeds to draw an exactly similar triangle, which it accurately superimposes on the first, and so on, to any number of triangles. The number of points in the figure is regulated by previous adjustment of the constants of the machine; but the mass of the instrument, its rate of motion, and the number of times it is resolved (beyond the construction-minimum) have nothing to do with the resulting figure. The definite proportions of chemistry, in like manner, precede or accompany each of our experiments ; they are independent of mass, rate of action, repetition of action; and doubtless they are produced, like these points, by compound uninterrupted motion. They certainly suggest nothing that is by nature atomic. These mathematical conceptions, however, involve no breach of continuity, which is rather their essential condition ; and the chemical phenomenon is at least as conceivable as they are without introducing the supposition of a limit.

I may adduce the process of diffusion as one that, in accordance with the large number of experiments already made, is probably capable of continuance without any clear reason for a limit. Yet if matter consisted of indivisibles, some sign, at any rate, of

a limit ought to have been by this time detected. The conti-
nuity of the gaseous and liquid states furnishes a strong experi-
mental presumption against any kind of constitution of matter,
corpuscular or otherwise. The atomic conception of definite
proportions is therefore not only not absolutely necessary, but
doubly improbable.

The law of definite proportions, indeed, is itself tinged with
continuity. It represents one side only of the series of bodies,
which includes mechanical mixtures on the one hand, definite
compounds on the other, and indefinite substances (like albumen)
as its middle term. Even should much more refined methods
of determining a symbolic value be discovered than we now
possess, the law of homology is an instant prophet of their
weakness; starting with almost perfect definition, it ever points
to some possible transcendent complexity.

Such is the nature of the arguments involved in ratiocination
upon a materialistic basis. Matter, it is asserted, must be either
infinitely or finitely divisible, as if either conception had ever
been realized by the interlocutor. What if matter do not exist
at all ? And if it exist, where is the proof that it and division
have any mutual connexion whatever? It is these prior ques-
tions that chemists, as a rule, never raise, or dismiss as fruitless—
forgetting that in philosophy, the storehouse of the most general
propositions of all the sciences, they have their only court of
appeal. Under the impression of these convictions, I now pro-
ceed to give an abstract of Digby's* most able argument on
the nature of quantity, a subject which evidently involves the
atomic theory among its component questions.

In the first place, Digby investigates the meaning tacitly or
otherwise assigned to the idea of quantity by the learned as well
as the uninstructed. "If you ask what quantity there is of
such a parcell of cloth, how much wood in such a piece of timber,
how much gold in such an ingot, how much wine in such a
vessel, how much time was taken up in such an action ? he that
is to give you an account of them measureth them by ells, by
feet, by inches, by pounds, by ounces, by gallons, by pints, by
dayes, by houres, and the like ; and then telleth you how many
of those parts are in the whole that you enquire of.... Where-
fore, when we consider that Quantity is nothing else, but the
extension of a thing ; and that this extension is expressed by a
determinate number of lesser extensions of the same nature ;
(which lesser ones, are sooner and more easily apprehended then
greater ; because we are first acquainted and conversant with
such ; and our understanding graspeth, weigheth and discerneth
such more steadily ; and maketh an exacter judgment of then ;)

* On the Natvre of Bodies (1645), p. 11 *et seq.*

and that such lesser ones are in the greater which they measure, as parts in a whole ; and that the whole by comprehending those parts, is a mere capacity to be divided into them ; we conclude, That *Quantity* or *Bignesse* is nothing else but divisibility ; and that a thing is big, by having a capacity to be divided, or (which is the same) to have parts made of it.

" This is yet more evident (if more may be) in Discrete Quantity (that is, in *number*) then in continued Quantity, or extension. For if we consider any number whatsoever, we shall find the essence of it consisteth in a capacity of being resolved and divided into so many unities, as are contained in it ; which are the parts of it. And this species of Quantity being simpler than the other, serveth for a rule to determine it by : as we may observe in the familiar answers to questions of continued Quantity, which expresse by number the content of it : as when one delivereth the Quantity of a piece of ground, by such a number of furlongs, acres, perches, or the like."

Having thus ascertained, with his customary acuteness, the nature of quantity and two of its species, Digby proceeds to discuss a point which underlies the entire atomic controversy, namely, does quantity consist of parts, or is it one ?

" Ells, feet, inches, are no more reall Entities in the *whole* that is measured by them, and that maketh impressions of such notions in our understanding ; then . . . . colour, figure, mellownesse, tast, and the like, are several substances in the apple that affecteth our several senses with such various impressions. It is but one *whole*, that may indeed be cut into so many severall parts : but those parts are not really there till by division they are parcelled out : and then the *whole* (out of which they are made) ceaseth to be any longer : and the parts succeed in lieu of it ; and are every one of them a new *whole*.

" This truth is evident out of the very definition we have gathered of Quantity. For since it is *divisibility* (that is, a bare capacity to division) it followeth, that it is not yet divided : and consequently, that those parts are not yet in it which may be made of it ; for division, is the making two or more things of one."

The next step is to point out that if parts be considered to exist in quantity, this must consist of points or indivisibles, " which we shall prove to be impossible." For if quantity were divided into all the parts into which it is divisible, it would be divided into indivisibles ; inasmuch as nothing divisible, and not divided, would remain in it. And as all these parts are actually in quantity, quantity must consist of indivisibles. None of these parts has any necessary claim to be distinguished from the others so as to remain divisible while they become indivisible ;

hence *all* the parts in quantity must be indivisible. But this assertion is encountered by the following dilemma. If indivisibles make quantity, either a finite or infinite number of them must do so. If a finite number, select as an instance three indivisibles, add them together, and make a line (whose extent being only longitude is the first and simplest species of quantity, and therefore whatever is susceptible of partition must be at least a line). Now, by the conditions, this line cannot be divided into more parts than three; yet Euclid (Elements, VI. Prop. 10) has demonstrated that a line can be divided into any number of parts, however great. Therefore it is evident that no line, still less any more complex species of quantity, consists of indivisibles.

Now, Euclid's demonstration being universal, and proving that all extension can be divided infinitely into parts, we must needs confess that it is of the nature of indivisibles, when they coalesce, to be drowned in one another; for otherwise there would result a kind of extension out of them that would be *in*divisible, contrary to the demonstration referred to. But if these indivisibles (even if infinitely numerous) are drowned in one another, they shrink to a single indivisible point. On the other hand, the nature of extension requires that one part be not in the same place where the other is. Therefore quantity cannot consist of an infinite number of indivisibles; and it has been shown that it is not constituted of a finite number. The dilemma terminates; and it is proved that quantity does not consist of indivisibles (*either finite or infinite in number*), and, consequently, that *parts are not actually in it.*

With this answer to a molecular or atomic theorist, it might have been supposed that our acute and critical reasoner would have been content to relinquish the argument. Yet he lingers a little about it to discuss a difficulty, apparently raised by Sense:— Are there not parts in a man's body—for example, arms, legs, fingers, and toes? Digby points out that sense does not judge which is an arm, leg, finger, or toe, but that the notions corresponding to these words are products of the understanding, which, among several functions of a substance, is capable of selecting *one*, as if the substance had no more. We are, there-fore, really dealing with a fallacy of confusion. Quantity is a possibility to be made distinct things by division; whereas the different limbs above named "are but a virtue to do distinct things." Even if this were not so, sense cannot determine any one part in a body; for if it could, it would tell precisely where that part begins and ends. If the part begin or end in indivisibles, certainly sense cannot determine of them. On the other hand, considering that all whereof sense is capable is divisible, it continually reminds us of more potential parts than one.

Hence it knows with exactitude neither the One nor the Many.

The special form of fallacy that underlies the atomic theory is the one known as the materialistic. Just as those who assert the existence of matter declare or imply their belief in it as a necessary substratum for hardness, weight, and other properties, which are really their own sensations ; so those who advocate the atomic theory, declare or imply their belief in indivisibles, in units of affinity, and so forth, as substrata. On close examination it is found that the substratum referred to has its necessity only in the imagination ; and the atomic theory, which has seldom even pretended to adduce experimental evidence for what it affirms to *exist*, is traceable to the same parentage. The same impatience at the recondite nature of forces which we cannot see, which has led many of us to assert so much more than we know, has caused both civilized and savage nations to clothe in statuary their invisible gods. The same impatience, mingled with discontent, is recognizable in that passionate craving for the absolute, or complementary in existence, which is common to mankind. Surrounded on all sides with continuity, motion, and change, our most popular ideas relate to limits, repose, and stability. With the latter, the atomic theory perfectly accords ; it readily blends with all the prejudices of our education, and is reinforced by them ; so that after some years it becomes an essential part of the mind, which has no longer the power to reject it, even with the aid of the desire. How hard it is, even for a young idealist, to be content with Nature as she is ! Such considerations show the subtlety of atomicism, and account for its long and obstinate survival.

One of the most remarkable points in connexion with the history of the atomic theory is the manner in which the actual realization of the atom has been kept in the background. It would be a matter of the highest importance, one would imagine, especially on the part of experimental advocates, to adduce, or at any rate to endeavour to adduce, an atom itself as the best proof of its own existence. Not only has this never been done, but no attempts have been made to do it ; and it is probable that the most enthusiastic atomist would be the first to smile at such an effort, or ridicule the supposed discovery. For who has ever seen that which he cannot divide ? or who, being unable to divide, would not at once suspect a defect in his tools rather than indivisibility in the substance submitted to experiment ? The experience of two thousand years has failed to produce or to discover a single atom of the innumerable millions that have existed during the whole of that time, and still remain unhurt in their modest retirement. Yet, in the present day, they are

spoken of as familiarly as if they were fossils. The ingenuous confession of Gregory is difficult to reconcile with his practice, but it doubtless expresses the feeling of many atomists : " we may admit the atomic theory. It is to be observed, however, that we have no positive proof of its truth, nor are we likely to obtain such proof." Of what use is it to serve a writ of *habeas corpus* upon such a theory ?

At this point my mind naturally recurs to a period in the history of chemistry when the science was in one particular re spect under as anomalous a régime as prevails at present. From about the year 1700 the belief in *phlogiston* prevailed for three quarters of a century, and was as intimately associated with experimental inquiries as the atom is now ; and this belief, as any one who has read a fragment of the literature of that epoch must have observed, was as real and living as can be con ceived. Yet, in 1764, Macquer * confesses, " Hitherto chy mists have never been able to obtain the phlogiston quite pure and free from every other substance ;" and Bishop Watson, in 1800 (1781 ?), with a somewhat ruffled dignity, exclaims†, " You do not surely expect that chemistry should be able to pre sent you with a handful of phlogiston separated from an inflam mable body ! " In course of time the nature of phlogiston varied as much as the atomic weight of any modern element. It began with being *materia vel principium ignis, non ipse ignis,* and ended with a narrow escape from identification with hydro gen. It was as material as Lavoisier's *caloric.* Yet chemists argued universally, and apparently by preference, without any quest of the substance itself. How the native freedom of an ex perimental science could coexist in the same person with so marvellous an abrogation of that freedom was long a puzzling phenomenon to me. The phlogistian, however, is much sur passed by the atomist. The former never ascribed to his veiled companion an attribute without natural analogy ; but the latter teaches Tertullian's paradox, *Certum est, quia impossibile.*

When a great system, instead of reposing on clearly ascer tained facts, is built upon the sands of fancy, its history must, as a rule, be a record of feeble instability. What has been the case with chemical theory so far as it has been atomic ? The absolute, unalterable carbon atom has weighed 6, 3, and 12 ; that of oxygen was 8 a few years since, now it is 16; but a con temporary chemist proposes to return to the old number. At first the atoms are introduced with radiant rods or lines of at traction ; then these appendages appear to have become super fluous, for we hear nothing of them for many years ; now they are to be seen on numerous lecture-tables, are treated of in most

* *Traité,* i. p. 10.   † Essays, i. p. 167.

systematic works, and it is the object of the majority of chemical researches to develope formulæ in connexion with them. Our modern theorist " explains " the existence of the two bromides ($C^3 H^5 Br$, $C^3 H^5 Br^3$) on the principle of atomicity; while the latter of their two analogues ($I Cl$, $I Cl^3$) is a " molecular combination." This is certainly the most pitiful evasion that ever passed under the name of theory. Again, the hypothesis of specific volumes, which represents a natural fact, is now in a languishing state because we cannot determine how many atoms and how much space make up a given volume of a substance.

It is not difficult to show that the doctrine of atoms involves several important contradictions, in addition to those previously alluded to in this tract. The theory of compound radicals shows conclusively, in its own way, that whatever compound radicals may do, is also done or imitated by absolutely homogeneous atoms; their functions are, in short, the same. Hence by the strict necessity of an inductive logic, as well as by the freedom of our nature, we are driven to deny that the atoms are atoms at all. For if the benzoyl atom ($C^7 H^5 O$) and chlorine atom (Cl) agree in so many of their functions, the probability is that they will agree somewhat in another, namely, complexity. The atomic theorist, however, is too anxious to save himself to allow this conclusion. Reversing history, he takes the chlorine atom first, compares the benzoyl atom with *it*, and asserts that the radical is a *compound atom*. To have asserted that the atom is a compound radical, would merely have been to narrate the course of a probable discovery; to pronounce the radical a compound atom is to state a pure contradiction, which is, by the laws of language, completely unintelligible.

These are the parts of a system which has been termed, in modern times, an exact science! It is consoling to remember that the most fanciful period of the phlogistic theory immediately preceded the epoch when that theory was extinguished— when chemists, free for the moment from the fetters of a doctrine which called every operation a combustion, perceived the whole of nature to be open to their inquiries. Such a reform and such a liberation are needed, and may be hoped for now.

This memoir would be very incomplete were I to leave unnoticed the opinions of some of the more gifted thinkers with regard to the atomic theory. Newton, for example, is often referred to as an authority on the side of the atomist. That philosopher, indeed, considered it probable that " God, in the beginning, formed matter in solid, massy, hard, impenetrable, moveable particles " which never wear or break to pieces, " no ordinary power being able to divide what God himself made One

in the first creation"*.   He also followed Lucretius in asserting
that nature would not be lasting were the properties of these
particles other than he supposes them to be.   It will be observed
that Newton allows of indivisibility only so far as ordinary or
natural forces are concerned.   Elsewhere, in a passage too often
overlooked, he limits the indivisibility to his own time.   " But
whether these parts, distinct and as yet undivided by natural
forces, are able to be divided and sundered in their turn is un-
certain"†.   And again, he allows that " space is divisible *in in-
finitum* "‡.   It appears, then, that Newton had hardly reached
so great a degree of decision upon this subject as is commonly
believed.

Descartes has also been reckoned among the atomists, pro-
bably more from the circumstance that he was a strong systema-
tizer and formalist than on the basis of any of his extant works.
That Descartes himself would not have approved of such an im-
putation is clear from the following (translated) passage § :—"It
is also very easy to recognize that there can be in it " (sub-
stance) " no atoms, that is to say, parts of bodies or matter
which are by nature indivisible, as some philosophers have ima-
gined ; inasmuch as, however small we may suppose these parts
to be, yet, since they must be extended, we see there is not one
of them that cannot be further divided into two or more others
of smaller size, and hence is divisible."   He then goes on to
argue that it would be impossible, at any rate, to restrict the
power of the Deity in division ; and adds that it is of the nature
of even the smallest extended particle in the world to be divisible.

Leibnitz, moreover, has left on record not a few decided
expressions of opinion with regard to this subject.   Thus he
asserts‖ that "a material being could not be at the same
time material and perfectly indivisible or endowed with real
unity " (p. 580).   " Material atoms are contrary to reason, be-
sides being still composed of parts ; for the invincible attach-
ment of one part to another (if we could reasonably conceive or
suppose such a thing) would not destroy their diversity" (p. 584).
" Limits are viciously assigned to division and subtleness as
well as to the richness and beauty of nature, when atoms and a
void, or certain prime elements, including the Cartesian, are set
up in the place of true units, when the infinite is not recognized
in every thing, and the exact expression of the greatest in the
least . . . ." (p. 599).

Few among us would deny a hearing to Kant on a question
of which philosophy has, from the earliest times, constituted so

* Horsley's Newton, vol. iv. p. 260 *et seq.*
† *Principia*, vol. iii. p. 358 (1713).   ‡ Horsley, p. 263.
§ Cousin's Ed. (1824) vol. iii. p. 137.   ‖ *Œuvres* (Jacques), vol. i.

large a part. In his 'Observations on the Second Antinomy of Pure Reason'*, where the composition of substance is discussed, the following decisive remarks occur. "These objections" (to the infinite subdivisibility of matter) " lay themselves open at first sight to suspicion, from the fact that they do not recognize the clearest mathematical proofs as propositions relating to the constitution of space, in so far as it is really the formal condition of the possibility of all matter, but regard them merely as inferences from abstract but arbitrary conceptions which cannot have any application to real things. Just as if it were possible to imagine another mode of intuition than that given in the primitive intuition of space ; and just as if its *à priori* determinations did not apply to every thing, the existence of which is possible from the fact alone of its filling space. If we listen to them, we shall find ourselves required to cogitate, in addition to the mathematical point, which is simple,—not, however, a part, but a mere limit of space—physical points, which are indeed likewise simple, but possess the peculiar property, as parts of space, of filling it by their aggregation. I shall not repeat here the common and clear refutations of this absurdity which are to be found everywhere in numbers ; every one knows that it is impossible to undermine the evidence of mathematics by mere discursive conceptions . . . . It is not sufficient to find the conception of the simple for the pure *conception* of the composite, but we must discover for the *intuition* of the composite (matter) the intuition of the simple. Now this, according to the laws of sensibility, and consequently in the case of objects of sense, is utterly impossible."

Davy and Wollaston both refused to accept the atomic theory. Coming still nearer to our own time, I mention, in the last place, a great investigator who has but recently departed from among us. No one could be more acute, and certainly none more clear, than Faraday in the kind of reasoning that precedes discovery. This eminently successful acuteness and clearness were not, however, acquired by the aid of graphic formulæ or wooden models, nor were they accompanied by the doubtful support of any such material assistance. As the air grew thinner, his wing was broadened and its thrust more strong. In 1844, Faraday published† a 'Speculation touching Electric Conduction and the Nature of Matter,' in which the principal subject of discussion is the atomic theory. Starting with the datum, admitted by atomists, that matter consists of atoms and space, he observes that this is " at best an assumption ; of the truth of which we can assert nothing, whatever we may say or think of its proba-

* Meiklejohn's Translation of the Kritik, p. 275.
† Phil. Mag. S. 3. vol. xxiv. p. 136.

bility. The word atom, which can never be used without in-
volving much that is purely hypothetical, is often *intended* to be
used to express a simple fact; but, good as the intention is, I
have not yet found a mind that did habitually separate it from
its accompanying temptations; and there can be no doubt that
the words definite proportions, equivalents, primes, &c., which
did and do express fully all the *facts* of what is usually called
the atomic theory in chemistry, were dismissed because they
were not expressive enough, and did not say all that was in the
mind of him who used the word atom in their stead; they did
not express the hypothesis as well as the fact. But it is always
safe and philosophic to distinguish, as much as is in our power,
fact from theory; . . . . . and considering the constant tendency
of the mind to rest on an assumption, and, when it answers every
present purpose, to forget that it is an assumption, we ought to
remember that it, in such cases, becomes a prejudice, and inevi-
tably interferes, more or less, with a clear-sighted judgment."
Now, of the two constituents of matter, space is the only
continuous one. Consider, then, the case of shellac, a non-
conductor. Space, in it, must be an insulator, whatever the
atoms may be; for if it were a conductor, the shellac could not
insulate. But now take the case of platinum, which must also
be composed of atoms and space. Since platinum is a conductor,
space, being its only continuous constituent, must be a conductor.
Space, which is everywhere uniform, is therefore a conductor
and a non-conductor. "Any ground of reasoning which tends
to such conclusions as these must in itself be false." The facts
do not warrant any other conclusion than that what we call
matter is *continuous*. Indeed "a mind just entering on the
subject may consider it difficult to think of the powers of matter
independent of a separate something to be called *the matter*; but
it is certainly far more difficult, and indeed impossible, to think
of or imagine that *matter* independent of the powers. Now the
powers we know and recognize in every phenomenon of the crea-
tion, the abstract matter in none; why then assume the exist-
ence of that of which we are ignorant, which we cannot conceive,
and for which there is no philosophical necessity?"

If we must assume at all, let us assume as little as possible.
The system of Boscovich is in these respects superior to the
atomic; it assumes much less, and does not contradict the facts
of nature. In it Matter and the atom disappear; and we find
that substances are constituted of centres of force, attractive and
repulsive. For the *shape* of the atom, the direction and relative
intensity of these individual forces are substituted. "Matter is
not merely mutually penetrable, but each atom" (centre) "ex-
tends, so to say, throughout the whole of the solar system, yet
always retaining its own centre of force."

Here Faraday proclaims himself at once a Leibnitzian and idealist. No answer, so far as I am aware, has ever been made to his argument; nor did he, in the exuberant development of atomic suggestions which surrounded his later years, publish any retractation of his previous opinion. Therefore, that matter is *force, in some way determined,* was probably that great thinker's final belief.

Such being the condition of the atomic theory, whether regarded in its chemical or philosophical aspect, certain practical results follow. I shall not take into consideration the physical molecules, which are "invested with an arbitrary system of central forces invented expressly to account for the observed phenomena," and are hard—nor the new alternative of ring-vortices, which are not hard*. A logical mind, still free to make an effort, cannot be content to accept that theory without question, or to entertain it without suspicion. And on inquiry it will find, if my argument be sound, that the atomic theory has no experimental basis, is untrue to nature generally, and consists in the main of a materialistic fallacy, derived from appetite more than from judgment; while, on the other side, arises the IDEA OF MOTION, with its subordinate laws, true both to nature and the life of man, the highest product of the scientific and the pure reason, and the noblest generalization the world has yet known, because it is the only one that neither limits nor enslaves. So serious is your crisis, so momentous your decision.

12 Pemberton Terrace,
  St. John's Park, N.

XVI. *On the Constitution of Milk and Blood.*
By M. Dumas†.

DURING the most troubled years of the first French revolution, the old Academy of Sciences of Paris having been suppressed, its members none the less continued their patriotic cooperation in the labours required by the new necessities of the country. History has given them credit for this. It associates the names of the principal of them with those of the illustrious administrators and generals, who then caused the integrity of the French soil to be respected.

The editors of the *Annales de Chimie,* who had been compelled to suspend their publication under the reign of Terror, on resuming it had the happy thought of collecting, in two volumes, all the memoirs or reports with which the Academicians had been charged. In running through these we appre-

ciate at a glance the importance of the questions which were addressed to them, the insufficiency of the means at their command during those troublous times, and the merits of the practical solutions which they presented to the country, as the fruit of their previous studies, or of their improvised experiments.

Saltpetre, gunpowder, steel, weapons, gun-metal, potash, soda, soaps, paper, assignats, and many other objects implicated in the defence of the country, the working of its manufactures and the necessaries of life, gave occasion to investigations and discoveries of which the factories have not yet forgotten the tradition.

The siege of Paris by the Prussian army could not, it was said, be sufficiently prolonged to raise any questions of the same kind; but nevertheless it has been necessary, as in the time of our fathers, to seek for nitrated earths, to produce gunpowder, to manufacture and work up steel, to obtain bronze and cast cannon; we also have been in want of paper, and of a great number of useful objects.

Considerable, although rapid, investigations have been accomplished; and it will be useful as well as just not to allow their memory to be lost. I have busied myself in collecting the materials for this publication, which I shall carry out as soon as circumstances will permit.

Among the privations which our forefathers did not know in their most cruel intensity, those which caused the most decided sufferings to the existing population, relate to the want of combustibles, which was rendered intolerable and most destructive by an exceptionally rigorous winter—to the scarcity of milk and eggs, the certain cause of the premature decease of a great number of young children—and, finally, to the exhaustion of the supplies of corn, flour, and meat, which, rendering the capitulation of Paris inevitable, marked the precise day for it.

Three questions, which have occupied the mind of every man curious to foresee the future of science, were thus incessantly presented to the meditation of the scientific men shut up in Paris, not as far-away dreams in which the imagination delights and disports itself, but as the despairing prayers of a people in utter extremity:—

1. To obtain available heat, without combustibles;
2. To reconstruct food with mineral materials, without the cooperation of life;
3. To reproduce, at least, the essential food of man with non-alimentary organic materials.

Man, in warming himself by means of combustibles furnished

either by the existing vegetation, or by the remains of the ancient vegetation of the globe, and in nourishing himself by means of products obtained from plants and animals, demands every thing from life; but could he dispense with life in obtaining his combustible and his nutriment? Would the forces of science alone suffice to assure to him, in this urgent need, those satisfactions which he could no longer demand from the forces of living nature?

This was the question. If put in a time of peace and in the midst of abundance, it would probably have received more than one response in the affirmative. The progress of the physical sciences has been so brilliant! One is so much disposed to exaggerate their power! Electricity opens up such seductive perspectives! Synthesis has produced so many marvels in the hands of chemists!

If the necessity had not been so pressing, so that the question might have been raised as a philosophical thesis, and we could have said to the physicists and chemists, Could you not, if it were necessary, furnish man with heat and food without having recourse to plants and animals? how many, without saying *yes*, would, at least, have answered with one of those smiles which do not say *no*.

But in a crisis where it was necessary to realize immediately what would have been left to hope, people showed reserve; radical solutions were adjourned, and there was no question either of heating Paris without combustibles, or of feeding it without organic aliments.

But could organic materials usually disdained be converted into aliments, so as to replace, by means of clever combinations, those natural products which could no longer be procured?

It is not my design to notice what viands were served at table, or what resources we were led to seek in the blood and offal of the slaughter-houses which are usually thrown away, the bones, feet, and even the skins of the cattle slaughtered. Nor will I examine how the butter and lard, which were speedily exhausted, were replaced. Of these improvised arts some have disappeared with the circumstances which gave them birth, whilst others have left some useful teachings.

I shall treat only of a special question, the solution of which involved certain principles which it seems to me to be important to guard. Was it not possible to come to the assistance of new-born children by replacing the milk, which could no longer be got, by some saccharine emulsion? In this case there was no question of creative chemistry, but only of culinary chemistry. Recipes were not wanting, all reproducing an albuminous liquid, sugar, and an emulsion of a fatty body.

As a provisional succedaneum this artificial milk deserved to be welcomed. But sometimes there was such a conviction in the authors of these propositions, that one was forced to dread for the future the effects of their faith. This was of a nature to make too many proselytes, to the great injury of the children at nurse, and the great profit of the dealers in milk. How could the latter have the least scruple when they were taught to manufacture an emulsion which they saw recommended to the consumers, and even to mothers, as the real equivalent of milk ?

The services rendered by concentrated milk during the siege were too important to render any excuse necessary in the country which produces it, when we insist upon the preference always due to natural milk, as also upon the characters which at present do not permit us to confound any artificial milky liquid whatever with the truly secreted product.

Natural milk forms a liquid containing salts, sugar, caseum in solution, and fatty globules in suspension. Let us first see whether we can imitate these fatty globules by dividing or making an emulsion of an oily or fatty matter in a viscous liquid.

I believe that I experimentally demonstrated the contrary some years ago by showing that the globules of fatty matter of milk are protected from certain physical or chemical reactions by a true membranous envelope. Admitted by some, and disputed by others, the existence of this membrane seeming to me to be real and proven, there could be no question, in my opinion, about confounding an artificial emulsion with naked fatty globules with milk from the mammæ, presenting fatty globules enveloped by a membrane, true free cells, filled with butter, analogous to the agglutinated cells of adipose tissues.

The existence of this membrane may be proved by two chemical experiments.

The first depends upon the property possessed by sulphuric ether of dissolving fatty matters and collecting together those which are suspended in liquids, provided that they are free. Now if, after shaking together in a tube fresh milk and ether, they are left to rest, the ether floats on the surface without having dissolved any thing, and the milk resumes its place below the ether without having lost any thing of its appearance, or yielded any of its buttery matter.

But when subjected beforehand to the action of acetic acid, which is able to dissolve the envelopes of its fatty globules, milk, when shaken up with ether, loses its opacity, and yields its butter to that liquid, in which it may be found.

An inverse test leads to the same conclusions. A neutral salt, such as sulphate of soda, added to milk, enables us to filter it, and to retain upon the filter the globules of butter, whilst the serosity flows off perfectly limpid and clear. If the washings with saline water be continued, these globules may be freed from all the soluble products of the serum. Now if the butter consisted of simple fatty globules, there would then remain with them no trace of albuminous or caseous matter. But whatever care may be taken to prolong the washings, we always find with the fatty matter such a proportion of albuminized substance that there can be no doubt that it has remained there in the form of those envelopes or cells which constitute the globules of butter.

The microscope, moreover, shows plainly the constitution of the globules of butter, and reveals the constant presence of the envelopes. It is sufficient to crush the globules of milk by means of the compressor, to obtain a conviction that, after the spreading of the fatty matter, the butter-cell still retains its form and outline, thus showing that the contents and the container have each their distinct existence.

For these reasons, and for many others (for no conscientious chemist can assert that the analysis of milk has made known all the products necessary to life which that aliment contains), we must renounce, for the present, the pretension to make milk, and especially abstain from assimilating any emulsions to this product.

Besides we cannot have too much reserve where we have to pronounce upon the identity of two products, one natural, the other artificial, if they are not crystallizable or volatile—that is to say, definite. We can never affirm that we have reproduced a mineral water, or sea-water for example. When manure for plants, or aliments for man and animals are in question, is not the same reserve still more imperative?

These indefinite natural mixtures contain substances which the coarsest analysis discovers, with others less strongly characterized or less abundant, which are only revealed by delicate chemistry, and others again, and perhaps the most essential, which still escape us, either because they exist in infinitesimal proportions, or because they belong to the category of bodies which have not hitherto been distinguished from other chemical species.

It is therefore always prudent to abstain from pronouncing upon the identity of these indefinite mixtures employed in the sustenance of life, in which the smallest and most insignificant traces of matters may prove to be not only efficacious, but even indispensable. In proportion as science extends her

domain, we are sure to see the demonstrations of the appropri-
ateness of this reserve multiplied.

Among the fine investigations executed in France by those
who have continued the labours which occupied the life of the
illustrious Théodore de Saussure, the important thesis of M.
Raulin upon the vegetation of *Aspergillus niger* will always
be placed in the foremost rank. All the conditions of the
life of this Mucedinean have been so well determined by that
author that it may be cultivated with precision in a soil
formed of definite chemical species, as if we had to do with
the formation of a compound; and the soil once sown, we may
follow the transformation or the employment of each of the
elements necessary to its life, just as if we had to do with the
development of an ordinary chemical equation.

Now, who could have foreseen that the *Aspergillus niger*,
which has just made its appearance, for example, upon a slice
of lemon exposed to the air, required for the fulness of its
existence traces of *oxide of zinc*? How, after this, can we
doubt, in the case of plants of a higher order and especially of
animals, that, besides their coarsely appreciable aliments, they
require also traces of many other aliments, more delicately
used but not less necessary?

Milk has often been compared to eggs, both from a che-
mical and a physiological point of view. Their mission is
equally to furnish the young animal with the nourishment of
its earliest age; and they have as a common character that
they present in union a fatty matter, an albuminoid substance,
a saccharine or amylaceous matter, and salts.

But the egg possesses a vitality, an organization, of which
chemistry furnishes no evidence, and which the most minute
anatomy would be powerless to reveal. If fecundation had
not rendered manifest, by the rapid phenomena of segmenta-
tion which take place in it, that the mass of the yelk of an
egg is endowed with life, and that it obeys the impulsion of
the living germ which takes possession of it, we should still
be ignorant that the yelk of the egg is not a mere emulsion of
inert fatty matter.

Is not milk in the same case? One is led to think so
when we see that the yelk of the egg and milk have the same
destination and the same configuration, and that, if the yelk
obeys the action of the germ which is nourished by it, milk,
for its part, proves to be singularly ready to receive and
nourish germs of more than one kind, which, on reaching it,
become developed and live at its expense.

The power of synthesis of organic chemistry in particular,
and that of chemistry in general, have therefore their limits.

The siege of Paris will have proved that we have no pretension to make bread or meat from their elements, and that we must still leave to nurses the mission of producing milk. If some illusions upon this point have found their way into the minds of persons ill-informed as to the true state of science, they are due to the dangerous play of words to which the expressions *organic chemistry* and *organic substances* lend themselves, when applied as these are indifferently to definite compounds such as alcohol or citric acid, which are unfitted for life, and to indefinite tissues, the seat of life.

The former (foreign to life, and true chemical species) are the only ones that synthesis has reproduced. The latter, which can be formed only under the impulse of a living germ, and which receive, preserve, and transfer the forces of life, are not definite species; the synthesis of the laboratories does not reach them. The only synthesis which has hitherto been observed in the case of the chemical materials which constitute living tissues, is that determined in brute matter by the presence and impulse of the living germ itself.

All those chemical syntheses, otherwise so worthy of interest, which have been indicated as reproducing organic matters, have therefore in reality reproduced only matters unfitted for life—that is to say, mineral matters. Thus, of every living matter or matter that has lived, we must still, whether we speak as chemists or as physiologists, say what was said of it formerly : *omne vivum ex ovo*—that which is not life has brought nothing to life.

With regard to the constitution of milk, the phenomena presented by the clarifying of butter have been sometimes employed either to demonstrate or to dispute the existence of the membranes which envelope the butyrous globules; I cannot at present regard these phenomena as having any value in this respect.

It has been said, for example, that the separation of butter was the result of the formation of lactic acid arising from the action of the air, favoured by churning. Numerous experiments effected in my laboratory upon a practical scale, have shown that butter separates equally promptly, and at least equally abundantly, from a milk to which a large amount of bicarbonate of soda has been added, as from natural milk. The alkaline reaction of the former, which is maintained during the operation and after its completion, has no influence either upon its duration or its result. The proportion of butter, far from being diminished, seems even to have been increased by it.

The formation of lactic acid is therefore not necessary for

the separation of butter, which appears to me to be due to purely mechanical causes. Such, at least, is the feeling that one experiences on examining by the microscope milk submitted to churning whilst the operation is going on. The first test-drops present nothing peculiar; the globules of butter retain their form, dimensions, and aspect. Soon we see appear irregular butyrous islands in the midst of globules remaining unaltered. These islands of butter increase in number and extent in proportion as the operation proceeds. They form a snow-ball, uniting with each other and becoming agglomerated so as to constitute, at last, the mass of butter which is the object of the operation.

The agglomeration of the butyrous globules into a block of butter would be a true regelation if there were no membrane surrounding them. The existence of this compels us to admit that it must be broken, and that this is the object of the repeated shocks which we make the liquid undergo, in order that the diffused butter may unite with the fatty parcels and agglomerations which it meets with on its road.

If it is true that the separation of butter is a purely mechanical phenomenon, it is not the less so, as I shall hereafter show, because chemistry can give rules to render this operation more rapid and more efficacious, and to produce from it a better clarified and less alterable butter.

I conclude this communication with some details upon phenomena of another nature, towards which the hygienic situation of the inhabitants of besieged Paris turned one's thoughts only too naturally. What took place in the tissues of this population deprived of fresh vegetables, fruits, milk, fish, and fresh meat? What changes did the blood undergo under the influence of this diet? and how must they manifest themselves?

Some years ago I had prepared some experiments the object of which was to ascertain whether exchanges by exosmose and endosmose take place between the internal liquids contained in the globules of the blood and the liquids of the serum. If these exchanges were easy and rapid, their existence might be ascertained. To demonstrate them would be to ascertain by what means the constitution of the globules of the blood may be altered and vitiated, reestablished, or regenerated.

I never completed these experiments; but I have often depended upon the views which guided me, in order to make my auditors in my courses at the faculty of medicine understand how certain alterations of the blood might be interpreted.

It is necessary, perhaps, to explain what stopped me.

Nothing is easier than to compare the serum and globules of a normal blood with the serum and globules of the same blood modified by the intervention of a substance capable of changing the direction or the intensity of the powers of endosmose between the globules and the serum.

In the blood of a living animal the globules suspended in the liquid may absorb or lose some of their elements, if we succeed in changing the constitution of the serum; but how long will the phenomenon last? If the substance added be mischievous it will be eliminated; the veins on their part will absorb liquids destined to reestablish the equilibrium, and the experiment will soon be so altered that the little differences that we have to measure will disappear, vanishing before great complications.

On the contrary, if we withdraw the blood from the body of the animal and divide it into two parts of equal weight, one destined to furnish the term of comparison, and the other to receive the substances modificative of the power of endosmose, coagulation and what I have called the asphyxia and death of the globules will soon do away with any hope of arriving at certain results.

It was therefore necessary to receive the blood into a vessel, to oppose its coagulation, and to replace towards it the action both of the heart and lungs—that is to say, to keep the blood in movement and to present it in a very divided state to the action of oxygen or of the air. I arranged an apparatus which fulfilled these conditions, and allowed one to ascertain how alcohol, neutral salts of soda or potash, sugar, &c. act when added to the serum, and how the interior liquids contained in the globules may become modified under their influence either in quantity or in nature.

While I followed out these views, preoccupied by the evident invasion of scurvy in the general state of health of the inhabitants of Paris towards the close of the siege, and whilst I sought to make up by applicable means for the absence of all fresh vegetables and of all fruit in their habitual diet, a foreign doctor, Dr. J. Sinclair, by following out the ideas which he had heard me teach upon this subject, was led to seek in them the explanation of the first symptoms of alcoholism, a state which he designates by the name of dypsomania.

Just as scurvy would have as its primary cause an impoverishment of the serum in potash-salts and a surcharge of salts of soda (which favours the exosmose of the potash of the globules and consequently their destruction), so alcoholism would have as its starting-point the presence of alcohol in the serum of the blood and its effects on the globules.

Alcohol added to the serum causes a movement of exosmose from the interior of the globules to the serum. The globules lose a part of their constituent liquids; and this alteration, which brings on others, is no doubt reproduced in the cells of the various tissues which are bathed by alcoholized liquids.

What it is now my intention to prove is, that in the blood in particular, and in every living organism of analogous constitution (that is to say, formed by cells or utricles filled with a liquid and floating in or bathed by a liquid), it is sufficient to alter, even slightly, the chemical composition of the exterior liquid to cause that of the interior liquid to become modified by endosmose or exosmose.

As soon as I am enabled to resume possession of my laboratory, if I should ever see it again, I propose to follow out the development and application of this principle, either to demonstrate the effects produced by the action of common salt, alcohol, &c. upon the blood, or to show how rapid is that of some agents, of which I have already examined the action, upon the constitution of the globules.

In the mean time I have yielded to the wishes of your eminent President, and I lay upon the table the exposition of those investigations which time may cause to fructify either in my own or more worthy hands. It is a homage that it is a pleasure to my old age to offer to that kind Society which, having, in 1816, guided my youth and the first steps of my career, offers me for the second time, in 1871, after an interval of half a century, the asylum of its friendly hospitality under grievous circumstances to my country.

XVII. *On the Mechanical Impossibility of the Descent of Glaciers by their Weight only. By* Henry Moseley, *D.C.L., F.R.S., Canon of Bristol, &c.**

IN various memoirs on the "Descent of Glaciers" which I have published in the Philosophical Magazine and elsewhere, it has been my object to show :—

1st. The mechanical impossibility of their descent by their weight only.

2nd. The actual cause of their descent to be their dilatation and contraction by alternations of temperature, in addition to their weight.

Mr. Croll[†], Mr. Matthews[‡], Mr. Ball[§], and M. Heim[||] have

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* Communicated by the Author.   † Phil. Mag. September 1870.
‡ Alpine Journal, February 1870.   § Phil. Mag. February 1871.
|| Poggendorff's *Annalen,* vol. v. (1870), and Phil. Mag. S.4. vol. xli. p.485.

done me the honour to reply to them. I am desirous to acknow-
ledge the courtesy with which the controversy has been conducted
by my opponents. Mr. Croll has done me the justice, I think, to
follow out the mathematical reasoning on which my argument is
based; and I will not complain if Mr. Ball has not. Mr. Croll
accepts that one of my two conclusions which is the subject of my
present paper. He concedes that "if the ice of a glacier be in the
hard, solid, crystalline state in which it is generally admitted to
be, and its particles shear in that state as they are assumed to do,
then glaciers cannot possibly descend by their weight only, as
is generally supposed, and the generally received theory of gla-
cier-motion must be abandoned"*. Mr. Matthews, Mr. Ball,
and M. Heim dissent from this conclusion. I propose to answer
their objections to it in this paper, and in a subsequent one ·
those they have made specially to my own theory of the descent
of glaciers.

The impossibility of the descent of glaciers by their weight
only, results from the consideration that, besides the motion of
the translation of a glacier bodily on its bed, there is a constant
displacement of its particles over one another and alongside
one another; for which displacement the work of a far more
powerful force is necessary than the weight of the glacier would
supply, it being subject (by the principles of mechanical philo-
sophy) to the condition that the aggregate *work* of the forces
which cause it must not be less than that of the resistances which
are opposed to it.

It is, of course, impossible to represent this inequality mathe-
matically in respect to a glacier having a variable direction and
an irregular channel and slope; but in respect to an imaginary
one, having a constant direction and a uniform channel and slope,
it is possible. I have computed it numerically in respect to a
glacier of a uniform rectangular section, slope, direction, and
roughness, descending with the same velocity as the "Mer de
Glace" does, and I have found that the work done by the weight
of the glacier in such descent through any distance is only
about $\frac{1}{55}$th of the work necessary to overcome the resistances
opposed to its descent through that distance.

The weight of this imaginary glacier would therefore be far
too small a force to cause it to descend as glaciers do descend.

The imaginary case to which my computation applies differs
from the case of the actual glacier in this respect, that the actual
glacier is *not* straight or of a uniform section and slope, and its
channel is not of a uniform roughness.

In all these respects the resistance to the descent of the actual
glacier is greater than to that of the imaginary one. But this being

* Phil. Mag. S. 4. vol. xl. p. 154.

the case, since in the imaginary glacier the weight is found to be insufficient to cause it to descend, much more must it be so in the actual glacier. This is my argument; and if it cannot be overthrown, it follows from it that no theory is to be received which accounts for the descent of glaciers by their weight only. The data I have used in my computation are Professor Tyndall's observations on the velocity of the surface-ice of the Mer de Glace at Les Ponts, and those on the velocity of the side-ice on the Glacier du Géant near the Tacul. These last observations were made under circumstances of great difficulty and some danger*. No conceivable error in the data can, however, account for the enormous disproportion which is shown to exist between the work of the force of gravity which is supposed to cause the glacier to descend and that of the resistances it has to overcome. Among these resistances I have reckoned those of the sides and bottom of the channel to be as great as though the ice were frozen to them; and considering what are the obstacles in the actual channel from projecting rocks, bends in its direction, and frequent contractions, the assumption of a resistance at least equal to that which would result in the imaginary form of glacier from the ice being frozen to its bottom and sides, is not perhaps unreasonable. My result is not, however, practically affected by throwing the resistance of the channel wholly out of the question. If its bottom and sides be conceived perfectly smooth, and if the ice of the glacier be supposed not to adhere to them at all, its differential motion remaining nevertheless unaltered, it still follows† that the work of the resistances through any distance of the descent is forty-seven times as great as the work of the weight of the glacier through that distance will supply.

The differential motion is, in point of fact, by far the greater part of the motion of the glacier—thirteen fourteenths of it on the Aar Glacier, according to Professor Forbes‡; so that the resistance to the differential motion, measured by its work, is by far the greatest resistance. Nor will any possible error in my assumed value of the unit of shear in ice affect the general result at which I have arrived. My experiments on it were made in a high temperature of the air. I have repeated them when it was below freezing, and found it greatly increased. As the temperature of the ice of a glacier is assumed not to be above freezing,

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* It is desirable they should be repeated at more leisure and with greater precautions than Professor Tyndall could command, and that the velocity of the surface-ice should be determined on the same section as that where the velocity of the side-ice is observed.

† Phil. Mag. May 1869. In equation (9), U_2 and U_3 are to be assumed to vanish; whence the above result will follow.

‡ Occasional Papers, p. 74.

we may conclude that the unit of shear in glacier-ice is not less than that given by my experiments, but greater than it.

Mr. Ball objects to my assuming the resistance of glacier-ice to be the same as that of the ice used in my experiments, which was the ordinary ice of commerce collected on the surfaces of rivers and lakes in Norway. " Glacier-ice," he says, " however slight the indications of it on the surface, is a congeries of separate fragments more or less perfectly welded together, and show-ing by the frequent presence of air-bubbles, and by its behaviour when exposed to radiant heat, an inferior degree of solidity " *.

But this is precisely the kind of ice with which some of my experiments on shearing were made. The ice was broken in pieces and then hammered into the cylindrical hole of the shear-ing-apparatus†, so as to be formed by regelation into a solid cylinder. It was then of the consistency of the glacier-ice de-scribed by Mr. Ball. It was visibly " a congeries of separate fragments more or less perfectly welded together." The glacier-grain of which M. Heim speaks was clearly seen in it. Its re-sistance to shearing was nevertheless almost identically the same as that of a cylinder of ice of the same diameter turned in a lathe out of a block of Norway river- or lake-ice ‡. The adherence of the new surfaces brought into contact in the act of shearing was as perfect in this granular ice as in that cut from the block ; and there was the same perfect continuity of the substance of the ice §.

As regards the influence of time on my determination of the unit of shear, I must have expressed myself imperfectly, for I have been misunderstood. In saying that it " cannot be deter-mined with absolute accuracy without taking into account the time of shearing " ‖, I meant that in the act of shearing a prism of ice in my apparatus, the dimensions of the surfaces which re-mained unsheared were continually diminishing, and with them the whole resistance to shearing ; so that to keep the conditions of the experiment always the same—the shearing-force being always just in excess of the resistance—the load must be con-tinually diminished. The velocity of shearing would then be constant. Or if, as in my experiments, the load remained the same, whilst by reason of the constant diminution of the surface of adherence the resistance diminished, the velocity of shearing would be continually accelerated ; and the degree of this accele-

* Phil. Mag. S. 4. vol. xli. p. 85.
† Phil. Mag. January 1870.   ‡ Ibid.
§ I believe this adherence of the new surfaces brought into contact in the act of shearing, which is so conspicuous a quality of ice, has been observed also in lead.
‖ Phil. Mag. January 1870.

ration, and therefore the time of shearing, would be dependent
on the form and dimensions of the section sheared and the unit
of shearing. The unit of shear in this case could only be deter-
mined by observing the time; and I have given a formula for
so determining it in the case of a prism having a rectangular
section*. In a temperature above freezing, the prisms of ice I ex-
perimented upon could not but have been in the act of melting,
and the whole section of shearing in the act of diminishing by
thus being melted; time must therefore in this way have had
an influence on my determination of the unit of shear. Heat,
too, must have been continually conducted to the ice through
the apparatus; and the process of shearing must have been ac-
celerated by the excess of pressure sustained at the upper and
lower surfaces of the prism, and the lowering of the temperature
of congelation, and therefore the melting at those points conse-
quent thereon. The section of the ice-cylinder was therefore
probably less than I have assumed it to be, and the unit of shear
greater.

Mr. Ball says very truly that "in some substances the amount
of resistance opposed to the separation of adjoining particles is
nearly independent of temperature, and of the time during which
the pressure is applied. In other bodies, which oppose a very
considerable resistance to fracture, the particles gradually change
their relative positions under the prolonged action of even a
slight pressure; and in some of them the change depends very
much on their temperature while exposed to pressure." Apply-
ing these remarks to my own determination of the unit of shear
in ice, I beg to remind Mr. Ball that it was made on one of the
hottest days (August 24) of the year 1869, when the thermo-
meter stood in the shade at 74°–75°, and that the ice I used was
almost in a state of deliquescence. No one who has inquired
into the temperature of glaciers has ever thought, I believe, of
assigning to it a temperature greater than this ice must have
had.

Ice covered compactly with other ice, as it is in ice-houses,
does not melt as this did. If the covering be perfect, as it is in
a glacier, it probably does not melt at all. So far as the influ-
ence of temperature is concerned, the unit of shear in glacier-
ice, however little below the surface, cannot, therefore, but be at
least equal to that of the ice in my experiments; far down in
the interior of the glacier it is probably much greater. I have,
however, brought this question of the shearing of ice in a tempe-
rature below freezing, and of the influence of time upon it, to
the test of a direct experiment. On the 15th of February, 1870,
I placed a cylinder of ice, one inch and a half in diameter, in a

* Phil. Mag. January 1870.

shearing-apparatus when the temperature was above freezing, and loaded it with the weights specified in the first column of the following Table.   It sheared through the distances specified in the second column in the times given in the third.

Load, in pounds.	Distance sheared, in inches.	Time, in minutes.
23	0	5
121	$\frac{1}{16}$	5
208	$\frac{3}{8}$	25
208	$\frac{1}{2}$	30
208	$\frac{5}{8}$	40
208	$\frac{3}{4}$	45
208	1	55

A second piece of ice, similar to the above, was placed in the same apparatus at $6\frac{1}{4}$ o'clock in the evening of the same day and loaded with $112\frac{1}{2}$ lbs., being at the rate of 63·6 lbs. per square inch.   At 8 o'clock in the evening the upper surface of the cylinder of ice was found to have yielded $\frac{1}{16}$ inch.   It was then left thus loaded all night. At $10\frac{1}{4}$ next morning it was examined again, and found not to have sheared at all during the night. The night had been frosty, but the frost had not been severe.

The unit of shear being given, the equality of the work through a given distance of the resistances, to the work through the same distance of the forces which cause the glacier to descend, whatever they may be, is independent of the length of time.   If the resistances work slowly, the forces which overcome them work also slowly and in the same ratio.   The conclusion I have drawn from this equality remains, therefore, unaffected by the question of time.

Mr. Matthews, however, and M. Heim after him, as well as Mr. Ball, deny that glacier-ice *shears* at all.   They say that it bends.   The following is Mr. Matthews's experiment as described by himself:—

"A plank of ice 6 inches wide and $2\frac{3}{8}$ inches in thickness was sawn from the frozen surface of a pond, and supported at each end by bearers exactly 6 feet apart.   The whole weight of the plank between the bearers could not have exceeded $37\frac{1}{2}$ lbs., and its cross section was nowhere less than 14 square inches.   According to the views of Canon Moseley, *shearing must surely have been impossible.*   Yet what was the result ? From the moment the plank was placed in position it began to sink, and continued to do so until it touched the surface over which it was sup-

ported. At the point of contact it appeared bent at a sharp angle, and was perfectly rigid in its altered form. The total deflection was 7 inches, which had been effected in about as many hours under the influence of a thaw under which the plank diminished very slightly in thickness. . . . . With this property of ice, viz. its power of changing its form under strains produced by its own gravitation, combined with the sliding movement demonstrated by Hopkins, we have, as it seems to me, adequate causes of glacier-motion"*.

"If," says M. Heim, after describing an experiment analogous to those of Tyndall on the moulding of ice by pressure, "if I rightly understand the experiments of Professor Tyndall and my own on the moulding of plates of ice, the ice therein and in the glacier *is not compelled to shear.*"

The idea present in common to the minds of these gentlemen seems to be that in bending so as to take a set, ice does not shear. In this I venture to think there is a misapprehension. In the bending of a plate of ice every particle, except those at the points of support, is made to move in the direction in which the plate is bent—those particles which are at the point of greatest inflection being made to move furthest, and those nearer to it being always made to move further than those more remote ; so that every particle moves over that which is alongside towards the nearest point of support ; and being assumed to have taken a set, it must have sheared over it. If the resistance to this shearing had been the only one to be overcome, then the aggregate work of the shearing of all the particles of the plate in the act of bending would have been exactly equal to the work of the pressure which bent it ; and in the case of a glacier, it would have been necessary to make (on these gentlemen's theory, that it descends by bending) the same calculation that I have made by equating the work of the resistances of its particles to shearing to that of the forces which cause it to shear ; from which there would have followed precisely the result at which I have arrived. But the resistances to *bending* are resistances to shearing plus other resistances at right angles to these. If, therefore, the weight of a glacier is insufficient alone to overcome the resistances to shearing, *à fortiori* " it is insufficient to overcome the resistances to bending.

The pressure theory supposes, however, not exactly that the ice is bent as in Mr. Matthews's experiment, but that it is first crushed or granulated by an exceedingly slow and imperceptible process of molecular crushing and granulation, and then that, being compressed, it is regelated. In the act of being thus moulded it shears, for the reasons above stated, and is dislocated

* Alpine Journal, No. 28, February 1870.

in other directions than that in which it is sheared. The forces necessary to cause a glacier to descend on the pressure theory are therefore those necessary to cause it to shear plus other forces necessary to complete its granulation; and as its weight has been shown to be insufficient to cause it to shear, as it actually does shear, *à fortiori* they are insufficient by granulation to mould it.

Mr. Matthews's experiment would moreover require to be greatly varied to bring it into analogy with the case of a glacier. If a glacier is supposed to descend by bending, the bending must be supposed to be in the plane of its surface or in a direction parallel to that plane, and not perpendicular to it, as it was in the case of the ice-plank. To make his experiment apply to the case of the glacier, Mr. Matthews should have placed his plank on the two bearers, not flatwise, but edgewise. Indeed, looking at the proportion of the length to the width of a glacier, and considering that it is in the direction of its length that the bend must take place if it descends by bending, the plank ought rather to have been placed vertically on one of its ends, and then it should have been observed whether a deflection showed itself endwise by lines on its surface similar to those of the veined structure and dust-bands in glaciers; and even if these appeared, the analogy would not have been complete unless they had been found also to exhibit themselves when the ice-plank, instead of resting vertically on one of its ends, had been inclined at an angle of $4° 53'$ to the horizon, being the inclination of a part of the Mer de Glace.

Of Mr. Croll's theory of the descent of glaciers I will speak when I advocate my own. With reference to my present argument, it is sufficient to say that it agrees with my theory in this, that it attributes the descent of glaciers to the work of their weight plus the work of the heat they are constantly receiving from without[*]. It acknowledges, therefore, the insufficiency of their weight alone to cause them to descend, which is my present argument.

In Professor Tyndall's experiment, solid ice was crushed and moulded by pressure; and in his theory the moulding of the solid ice of a glacier into its channel and its continuous motion along it, like that of a liquid, is accounted for by the pressure of its weight.

In Professor Forbes's theory, the insufficiency of its weight so to mould solid ice and to give it this continuous motion like that of a liquid is implied, and it is supposed to be a semiliquid. It is its weight which, according both to the pressure theory and the viscous theory, is the sole cause of its descent: the

former supposes its weight a force sufficient to do so in the solid state; the latter, to make it sufficient, half melts it.

If, according to my present argument, the descent of a glacier by its weight only is impossible, neither of these theories is admissible. Agreeing with the pressure theory as to the essential solidity of glacier-ice, and with the viscous theory as to the insufficiency of the weight of the ice alone to cause it to descend in the solid state in the stream-like way in which it actually does descend, I have ventured to propose a theory which calls in aid of its weight the dilatations and contractions of its mass caused by alternations of temperature.

To arrive at some knowledge, however imperfect, of the elasticity of ice from its deflection under given deflecting forces, I made the following experiments on the 28th of December, 1869, when the temperature of the air was considerably below freezing.

I caused planks of ice to be taken from the surface of a pond, and carefully sawn into rods and planed on a carpenter's bench and squared. It was very difficult to do this, because of the extreme brittleness of the ice. The surface nevertheless worked easily under the plane, and was quite dry (in that temperature of the air) and beautifully smooth. My first experiment was with a rod of ice $4\frac{1}{2}$ inches wide by $1\frac{1}{4}$ inch thick, which I placed horizontally on bearings 3 feet 3 inches apart. When loaded in

the middle with a weight of 12 lbs. it broke after deflecting $\frac{1}{16}$ inch. The fracture was probably caused by some motion given to the last instalment of the weight in the act of being placed upon it. The surface of fracture was continuous and symmetrical; and hav-

ing been repeated under the same geometrical form in all the similar experiments which I made, I append a diagram copied from a tracing of it.
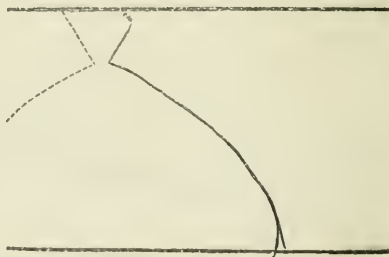
In a second experiment I placed a rod of ice, $1\frac{3}{16}$ inch broad by $\frac{1}{2}$ inch thick, horizontally upon bearings 3 feet apart—and beside it a rod of wood resting on the same bearings, and adjusted so as to serve as a straight edge. To this rod of wood a small ivory rule, divided into fiftieths of an inch, was fixed vertically to measure the deflections of the rod of ice in the middle. By its own weight alone, which was 11·713 oz., it deflected ·04 inch.

The following Table shows its deflections when loaded in the middle with other weights :—

Deflection of a Rod of Ice.

Index-number of experiment.	Load, in ounces.	Deflection, in inches.	Modulus of elasticity on the supposition that the elasticity of the rod remained perfect throughout its deflection.
1.	0	·04	4956300
2.	4	·06	5109200
3.	8	·08	5185800
4.	12	·10	5231600
5.	16	·12	5262300
6.	20	·14	5284400
7.	24	·16	5301600
8.	28	·18	5313600
9.	32	·20	5323700
10.	36	·22	5332000
11.	40	·24	
12.	44	·26	
13.	48	·28	
The weights were here taken off, and the permanent deflection or set of the rod was found to be ·10 inch.			
14.	54	·30	
15.	58	·32	
16.	62	·34	
17.	64	·36	
Permanent set ·12 inch.			
18.	66	·36	
19.	70	·38	
20.	74	·40	
21.	78	·41	
22.	82	·44	
23.	86	·46	
24.	90	·48	
25.	94	·50	
Permanent set ·18 inch.			

The rod finally broke with a load of 7 lbs. As each additional load was placed on it, it vibrated, showing its elasticity (within the narrow limits of vibration) to remain nearly perfect. Each additional weight gave it, however, eventually an additional set. The following seem, from these and other experiments, to be physical properties characteristic of ice :—

1. The preservation of the continuity of its substance when it is made to shear. I will venture to call this readhesion.

2. Its extreme brittleness.

3. Its elasticity at low temperatures.

4. Its tendency when deflected easily to take a set.

I have calculated the modulus of the ice-rod as shown in the first ten of the experiments, of which the results are given in the preceding Table, on the supposition that the elasticity of the

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rod remained perfect throughout.   Their values are recorded in
the fourth column of the Table.

That this perfect elasticity was not, in point of fact, pre-
served is plain from the variation of the modulus.   If it were,
and the experiments could in all respects be relied upon, the
elasticity of this rod of ice would be about one third that of cast
iron, three times that of deal, and a little greater than that of
tin*.

I am far, however, from putting these experiments forward
as sufficient in accuracy for determining a physical constant of
so much importance as the modulus of elasticity of ice.   The
elasticity of this ice-rod would have been different at a different
temperature—or possibly if it had been taken out of another pond,
or even if taken out of a different part of the same pond.   The
elasticity of such a rod of ice remains perfect only within certain
exceedingly small limits of deflection, which it is not easy to
observe with precision in the open air in a temperature consider-
ably below freezing.   To that end an apparatus better adapted
to the purpose than mine would be necessary.

Mr. Matthews has referred in just terms of respect to the
contribution of the late Mr. W. Hopkins to the science of
glacier-motion, and particularly to that well-known experiment
in which placing a block of ice on an inclined plane of sandstone,
the ice descended (melting at its surface of contact with the
sandstone) with a velocity which, when the inclination of the
plane did not exceed a certain limit, was uniform.

I have repeated Mr. Hopkins's experiments in various ways.
I have used rough paving-stones for the inclined plane, and
stones pierced with holes to allow of the escape of the water
melted from the ice at the surfaces of contact, which seemed
to lubricate them.   With the same view I have used soft sand-
stone to imbibe the water, and I have caused ridges to be
cut in its surface in the direction of the descent and across
it.   In all these variations of his experiments I have found a
perfect confirmation of Mr. Hopkins's results.   His experiments
shows it *not* to be mechanically impossible that *a block of ice*
should descend on an inclined plane by its weight alone—and
therefore not to be mechanically impossible that a glacier should
descend by its weight alone, if it descended as the block of ice
did in his experiment.   But it does not.   There is an essential

* The modulus of elasticity of hot-blast cast iron (Buffery) is, according
to Hodgkinson, 15381200; that of deal (Memel), according to Barlow,
1535200; and that of cast tin, according to Tredgold, 4608000.   I
think the best means of determining the modulus of elasticity of ice would
be found in the acoustic properties which at low temperatures it probably
possesses.

difference; and precisely in this difference lies the impossibility of the glacier descending by its weight alone. The block of ice descended bodily; its parts did not move over one another or alongside one another, but with a common motion of descent; whereas not less than thirteen fourteenths of the motion of the Glacier of the Aar is, according to Professor Forbes, not a common motion, but that of its particles one beside another and one over another, which is called a differential motion, and to which is opposed the resistance to shearing, which is at the rate of not less than 75 lbs. per square inch.

Mr. Hopkins's experiment leaves therefore more than thirteen fourteenths of the power necessary to cause a glacier to descend unaccounted for.

In conclusion I will venture to quote a passage from Mr. Croll's paper, although it expresses an opinion favourable to my present argument, because it states in terms more clear and explicit than I can do the little influence that alleged errors in the data on which my conclusions are founded would have, even if their existence were admitted, on the result at which I have arrived [*] :—" The impression left on my mind after reading Canon Moseley's memoir in the Proceedings of the Royal Society for Jan. 1869 was that, unless some very serious error could be pointed out in the mathematical part of his investigation, it would be hopeless to attempt to overturn his general conclusion as regards the received theory of the cause of the descent of glaciers, by searching for errors in the experimental data on which the conclusion rests. Had the result been that the actual shearing-force of ice is by twice, thrice, four times, or even five times too great to allow of a glacier shearing by its own weight, one might then hope that by some more accurate method of determining the unit of shear than that adopted by Canon Moseley, his objections to the received theory of glacier-motion might be met; but when the unit of shear is found to be not simply by three times, four times, or even five times, but actually by thirty, forty, or fifty times too great, all our hopes of overturning his conclusion by searching for errors in this direction vanish, even although there are some points connected with his unit of shear that are not satisfactory."

* Phil. Mag. S. 4. vol. xl. p. 154.

XVIII. *Proceedings of Learned Societies.*

April 27, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communications were read :—
   "On the Increase of Electrical Resistance in Conductors with rise of Temperature, and its application to the Measure of Ordinary and Furnace Temperatures ; also on a simple Method of measuring Electrical Resistances."—The Bakerian Lecture.   By Charles William Siemens, F.R.S., D.C.L.

The first part of this Paper treats of the question of the ratio of increase of resistance in metallic conductors with increase of temperature.

The investigations of Arndtson, Dr. Werner Siemens, and Dr. Matthiessen are limited to the range of temperatures between the freezing- and boiling-points of water, and do not comprise platinum, which is the most valuable metal for constructing pyrometric instruments.

Several series of observations are given on different metals, including platinum, copper, and iron, ranging from the freezing-point to 350° Cent., another set of experiments being also given, extending the observations to 1000° Cent.   These results are planned on a diagram, showing a ratio of increase which does not agree either with the former assumption of a uniform progression, or with Dr. Matthiessen's formula, except between the narrow limits of his actual observations, but which conforms itself to a parabolic ratio, modified by two other coefficients, representing linear expansion and an ultimate minimum resistance.

In assuming a dynamical law according to which the electrical resistance of a conductor increases according to the velocity with which the atoms are moved by heat, a parabolic ratio of increase of resistance with increase of temperature follows ; and in adding to this the coefficients just mentioned, the resistance r for any temperature is expressed by the general formula

$$r = a\,T^{\frac{1}{2}} + \beta\,T + \gamma,$$

which is found to agree very closely both with the experimental data at low temperatures supplied by Dr. Matthiessen, and with the author's experimental results, ranging up to 1000° Cent.   He admits, however, that further researches will be necessary to prove the limits of the applicability of the law of increase expressed by this formula to conductors generally, especially when nearing their fusing-point.

In the second part of this Paper it is shown that, in taking advantage of the circumstance that the electrical resistance of a metallic conductor increases with an increase of temperature, an instrument may be devised for measuring with great accuracy the temperature at distant or inaccessible places, including the interior of furnaces, where metallurgical or other smelting-operations are carried on.

In measuring temperatures not exceeding 100° Cent., the instrument is so arranged that two similar coils are connected by a light cable containing three insulated wires. One of these coils, "the thermometer-coil," being carefully protected against moisture, may be lowered into the sea, or buried in the ground, or fixed at any elevated or inaccessible place whose temperature has to be recorded from time to time ; while the other, or "comparison-coil," is plunged into a test-bath, whose temperature is raised or lowered by the addition of hot or cold water, or of refrigerated solutions, until an electrical balance is established between the resistances of the two coils, as indicated by a galvanoscope, or by a differential voltameter, described in the third part of the paper, which balance implies an identity of temperature at the two coils. The temperature of the test-solution is thereupon measured by means of a delicate mercury thermometer, which at the same time tells the temperature at the distant place.

By another arrangement the comparison-coil is dispensed with, and the resistance of the thermometer-coil, which is a known quantity at zero temperature, is measured by a differential voltameter, which forms the subject of the third part of the paper ; and the temperature corresponding to the indications of the instrument is found in a table, prepared for this purpose, in order to save all calculation.

In measuring furnace-temperatures the platinum-wire constituting the pyrometer is wound upon a small cylinder of porcelain contained in a closed tube of iron or platinum, which is exposed to the heat to be measured. If the heat does not exceed a full red heat, or, say, 1000° Cent., the protected wire may be left permanently in the stove or furnace whose temperature has to be recorded from time to time ; but in measuring temperatures exceeding 1000° Cent., the tube is only exposed during a measured interval of, say, three minutes, to the heat, which time suffices for the thin protecting casing and the wire immediately exposed to its heated sides to acquire within a determinable limit the temperature to be measured, but is not sufficient to soften the porcelain cylinder upon which the wire is wound. In this way temperatures exceeding the welding-point of iron, and approaching the melting-point of platinum, can be measured by the same instrument by which slight variations at ordinary temperatures are told. A thermometric scale is thus obtained embracing without a break the entire range.

The leading wires between the thermometric coil and the measuring instrument (which may be, under certain circumstances, several miles in length) would exercise a considerable disturbing influence if this were not eliminated by means of the third leading wire before mentioned, which is common to both branches of the measuring instrument.

Another source of error in the electrical pyrometer would arise through the porcelain cylinder upon which the wire is wound becoming conductive at very elevated temperatures ; but it is shown that the error arising from this source is not of serious import.

The third part of the paper is descriptive of an instrument for

measuring electrical resistance without the aid of a magnetic needle or of resistance-scales. It consists of two voltameter tubes fixed upon graduated scales, which are so connected that the current of a battery is divided between them, with one branch including a known and permanent resistance, and the other the unknown resistance to be measured. The resistance and polarization being equal, and the battery being common to both circuits, these unstable elements are eliminated by balancing them from the circulation ; and an expression is found for the unknown resistance X in terms of the known resistances C and γ of the voltameter, including the connecting-wires, and of the volumes V and V' of gases evolved in an arbitrary space of time within the tubes, viz. : —

$$X = \frac{V}{V'}(C+\gamma) - \gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Changes of atmospheric pressure affect both sides equally, and do not, therefore, influence the results ; but a reading at the atmospheric pressure is obtained at both sides by lowering the little supply-reservoir with dilute acid to the level indicated in the corresponding tube. The upper ends of the voltameter tubes are closed by small weighted levers provided with cushions of India-rubber ; but after each observation these levers are raised, and the supply-reservoirs moved so as to cause the escape of the gases until the liquid within the tubes is again brought up to the zero-line of the scale, when the instrument is ready for another observation. A series of measurements are given of resistances varying from 1 to 10,000 units, showing that the results agree within one-half per cent. with the independent measurements obtained of the same resistances by the Wheatstone method.

The advantages claimed for the proposed instrument are, that it is not influenced by magnetic disturbances or the ship's motion if used at sea, that it can be used by persons not familiar with electrical testing, and that it is of very simple construction.

"On the Change of Pressure and Volume produced by Chemical Combination." By M. Berthelot.

1. A singular question has arisen in the study of the gaseous combinations, viz. can the pressure be diminished in consequence of a reaction, at the moment it is accomplished, at constant volume, without loss of heat, so that the phenomenon of explosion comes from the excess of atmospheric pressure upon the inner pressure of the system, instead of coming from the inverse excess of the inner pressure ? The discussion of this question, however special it appears at first sight, leads to general notions concerning chemical combination.

2. The pressure depends upon the temperature evolved, and upon the state of condensation of the products. Let us determine this quantity.

Let t be the temperature produced by the real reaction, this being effected at a constant volume, admitting that the whole of the disengaged heat was employed in warming the products.

Let V be the sum of the volumes of the gaseous bodies in the initial system at $0°$ and $0^{m}\cdot760$.

At the temperature t, the final system contains in general certain gaseous bodies.

Let V′ be the volume of these bodies, supposed to be brought, without changing their state, to $0°$ and $0^{m}\cdot760$.

The relation $\dfrac{V'}{V}=\dfrac{1}{K}$ expresses the condensation produced by the reaction.

When certain bodies, contained in the initial system at $0°$, or in the final system at $t°$, are in the solid or the liquid state, you can generally neglect their volume in comparison with that of the gas, when the pressures are not too considerable.   Let us calculate the pressure during the reaction which takes place at a constant volume and at the temperature t, the initial temperature and pressure being $0°$ and H.

Admitting Marriotte's and Gay-Lussac's laws, the pressure will become

$$H \times \frac{1}{K}(1+\alpha t) ;$$

it will be greater than the initial pressure if $1+\alpha t > K$, less if $1+\alpha t < K$, or equal if $1+\alpha t = K$.   Let us observe that $t = \dfrac{Q}{c}$, Q being the quantity of heat produced in the reaction, and c being the mean specific heat of the products between $0°$ and $t°$.

Let us develope this solution.

3. The pressure augments when the condensation is null, for instance chlorine and hydrogen, $K=1$,—and especially when there is dilatation (combustion of acetylene by oxygen), t being always positive in a direct and rapid reaction between gaseous bodies.

4. On the contrary, the pressure diminishes if K is very great— that is, in the case of a system containing gaseous bodies transformed *entirely* into products which are in the solid or liquid state at the *temperature developed by the reaction.*   This case is more rare than one would think at first sight, because very few compounds subsist wholly at the high temperature that would be developed by the integral union of their gaseous components.   Generally a portion of these remain free at the moment of the reaction ; but in the present state of our knowledge it is impossible to estimate the pressure corresponding to effects so complex.

It is necessary to consider that the present case must not be confounded with the case in which the products formed in the gaseous state and at the temperature of the reaction are liquefied or solidified under the influence of a subsequent cooling ; for instance, the formation of water from its elements, or of chlorhydrate of ammonia from hydrochloric acid and ammonia, produces equally a diminution in the final pressure.

5. In theory the most interesting case is that in which the initial and final systems are wholly constituted of gaseous bodies whose

volume (calculated at $0°$ and $0^m \cdot 760$) is more condensed in the final than in the initial system. But this condensation is always comprised within very narrow limits, such as $K = 4$ (formation of arsenious acid by its elements), $K = 3, 2, 1\frac{1}{2}$, &c.; so the fundamental condition

$$1 + a \frac{Q}{c} < K, \text{ or } Q < 273 \, (K-1) \, c,$$

that determines a diminution of pressure, should be realized only in very exceptional cases and when the heat evolved by an integral reaction is very little.

One can ascertain it by making the calculation by means of the specific heats at constant volume (deduced with ordinary coefficient from the specific heats at constant pressure which M. Regnault has determined for many bodies). One can also make the calculation in a more general manner, by admitting with Clausius that the specific heats at constant volume have an identical value for the atomic weights of elements, that this value is equal to 2, 4, the number found for $H = 1$, and that it does not change from the fact of combination. W being the quantity of heat produced in a reaction between gaseous bodies calculated for atomic weights, and n the number of atoms in the reaction, the pressure will diminish only if

$$W < 655n \, (K-1).$$

It is easy to see that this condition is not fulfilled in the combinations best known. Calculating, either by means of this formula or by means of the preceding, I have not succeeded in discovering any example of diminution of pressure among the numerous reactions I have examined in this present research.

Besides, it is sufficient to make the calculation for the reaction supposed integral, the conclusion being generally the same for the reaction supposed incomplete—that is to say, in the case of dissociation, as it would be easy to prove.

6. Without further extending this discussion, I believe that a new general proposition relative to chemical combination can be deduced from it. It is known that every direct reaction which can be accomplished in a very short time between gaseous bodies with formation of gaseous compounds, produces a disengagement of heat: this is true for all reactions evolved by chemical forces alone, acting without help of any work done by exterior forces[*]. The new proposition is the following:—

The heat produced in a reaction of this sort, supposing it to be applied exclusively and without any loss to warm the products, is such that an augmentation of pressure always takes place at a constant volume, or, what is the same thing, an augmentation of volume at a constant pressure.

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[*] This proposition is contained in a more general one, which I have given in 'Annales de Chimie et de Physique,' 4me série, t. xviii.

This proposition results not from any *à priori* deduction, but is verified by the whole of facts known to this day.

7. One may ask if the change of volume, in which the gases keep the whole heat produced by their mutual actions, is regulated by a simple law, analogous to those that have been observed when the gaseous combinations are brought to the same temperature; nevertheless it does not appear to be so.

Let us compare the formation of the different hydracids by means of their gaseous elements, which gives no change of volume when the gas is reduced at 0° and $0^m\cdot760$.

The formation of chlorhydric gas, H Cl, produces 23,900 calories; the formation of bromhydric gas, H Br, produces 13,400 calories; the formation of iodhydric gas, HI, produces 800 calories. The specific heat of these gases being nearly the same under the same volume, it is clear that the quantities of heat aforesaid cannot produce an augmentation of volumes identical or proportional with simple numbers.

GEOLOGICAL SOCIETY.

February 22, 1871.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

The following communications were read :—

1. " On supposed Borings of Lithodomous Mollusca." By Sir W. C. Trevelyan, Bart., M.A., F.G.S.

2. " On the probable Cause, Date, and Duration of the Glacial Epoch of Geology." By Lieut.-Col. Drayson, R.A., F.R.A.S.

In this paper the author started from the fact that the pole of the ecliptic could not be the centre of polar motion, as the pole varied its distance from that centre. He indicated the curve which the pole did trace, and this curve was such as to give for the date 13,000 B.C. a climate very cold in winter, and very hot in summer for each hemisphere. The duration of the glacial epoch he fixed at about 16,000 years. The calculations resulting from this movement were stated to agree accurately with observation.

3. " On Allophane and an allied Mineral found at Northampton." By W. D. Herman, Esq.

In this paper the author gave analyses of an amorphous, translucent, reddish-yellow mineral, found incrusting sandstone in the Ironstones of the Northampton sands, the comparison of which with Mr. Northcote's analysis of allophane from Charlton leads him to infer the identity of the two minerals. He also noticed a soft white substance found in certain joints in a section of the Northampton sand, and also referred to allophane by the late Dr. Berrell, who analyzed it. This substance was said to occur not unfrequently in the Inferior Oolite of the Midland Counties. By analysis, it was shown to agree nearly with Samoite and Halloysite.

4. " Notes on the Peat and underlying Beds observed in the construction of the Albert Dock, Hull." By J. C. Hawkshaw, Esq., M.A., F.G.S.

The Albert Dock is situated on the foreshore of the river Humber. The excavations for the dock extended over an area of about thirty acres, and they were carried down to a depth varying from 8 feet to 27 feet below low water of spring tides.   Beneath the more modern deposits of Humber silt a bed of peat, Hessle Clay, Hessle Sand, and purple clay were successively met with.   The peat was found at the west end of the dock at the level of low water ; at the east end the bed dipped so that the upper surface was found at 8 feet below the level of low water.   In the peat were found the remains of a fire, which the writer attributed to human agency.   Oak-trees of large size were imbedded in the peat, some of which had grown where they were found, as was shown by the stocks remaining with the roots penetrating the Boulder-clay beneath.   In one oak-tree, 5 feet in diameter, a hole was found filled with acorns and nuts.   Many of the nuts were broken open at the ends, and had evidently formed part of the store of a squirrel.   Remains of Coleoptera were found, and one horn-core of a *Bos*.   The excavation did not extend below the upper parts of the purple clay.   Some of the borings, however, penetrated the chalk at a depth of 85 feet below low-water level, passing through a bed of sand 16 feet thick below the purple clay. Several thousand cubic yards of this sand were brought up into the foundations by springs of water which flowed up through old bore-holes.   The abstraction of this sand from beneath the clay-bed caused it to subside many feet.   The writer thinks that analogous subsidences may take place from natural causes—for instance, where large springs occur in tidal rivers.   Two sections exhibited showed the beds above the chalk for a distance of rather more than a mile along the foreshore.   The Hessle sand was shown to thin out to the westward.   It does not, in the writer's opinion, increase in thickness in that direction, as it was shown to do in a section already published in the Proceedings of the Society.

XIX. *Intelligence and Miscellaneous Articles.*

ON THE INFLUENCE OF A COVERING OF SNOW ON CLIMATE.   BY A. WOJEIKOF, MEMBER OF THE IMPERIAL RUSSIAN GEOGRA-PHICAL SOCIETY.

St. Petersburg, February 20, 1871.

THE influence of a layer of snow resting on the earth's surface in the colder portions of the earth during winter has, to my knowledge, never been considered in its general bearing on the climate and the conditions of the population living in these countries.

The first and most apparent influence of snow is the protection it affords to our crops from the cold of winter.   Where the snow-mantle appears regularly, winter crops are always sure, be the cold ever so intense.   In the steppes of South and East Russia ,

where little snow falls in winter, and this small quantity is often blown away by the strong winds, winter crops are scarcely attempted at all. On the northern coasts of the Black Sea, summer wheat and Indian corn are very good; but winter wheat is a precarious crop, while to the north, in Podolia, it is the principal crop. There the forests afford a protection against the wind, the snow falls more copiously and cannot be blown away.

As a bad conductor of heat, the snow isolates the warmer soil from the cold air above; and there is no doubt that it renders also the winter cold more intense, as the air cannot receive heat from below. In countries where the snow-covering is not permanent, as Western Europe, this influence of snow is well known, and people expect great cold where a layer of snow has fallen and the sky clears. In countries where snow usually lies the whole winter, as Russia and the northern parts of America, this is not generally understood, and in Russia people say it is colder without snow than with it. This feeling is quite natural. The first frosts of autumn are more severely felt because the human body is not accustomed to them, and also because the air is drier than with snow, and a cold dry wind is more severely felt than a cold moist one.

The great relative humidity of the air is a most important feature of the countries covered with snow in winter. It is as easy to account for it as for the humidity of an island in the middle of the ocean, or of a place situated in an extensive swamp-tract. The wind may come from every side, it has always to pass over a large evaporating surface, and absorbs moisture if it was originally dry. In countries where cold strong winds predominate, as in the greater part of North America and Eastern Asia, this will be less the case, as the winds, rapidly passing over the land, have not the time for absorbing much moisture; and the dryness of the air in the United States is felt by Europeans going there. But in countries situated like Europe and Western Asia, where the cold winds are usually weak, and only the warm southerly winds strong, the air will be always nearly saturated when the soil has a snow covering, as the cold winds in their slow progress have the time for absorbing moisture. This feature of climate is extremely important in the examination of storms. It was one of the chief merits of Espy to have pointed out the importance of vapour in the origin and progress of storms; and this is now generally admitted. If a storm is signalized and the beginning of its path stated, it is important to know the quantity of vapour disseminated in the countries where it is likely to pass, and the quantity which may be expected to be condensed as rain and snow. Now, the lower the temperature falls, the more uncertain are observations of the psychrometer; and I am of the opinion that it it not a sure guide below the freezing-point. This stated, it is very important to have some general idea as to the quantity of vapour over the cold spaces of the earth's surface. Now in countries situated like Europe, relative humidity will scarcely fall below 75°–80° so long as the earth is covered with snow, so that the quantity of vapour in the air of these regions may be very nearly known if we know the tempera-

ture.   In an examination of the barometric range in European Russia
and Siberia, some time ago, I have stated that not only does the pres-
sure of the air rise in winter as we advance from the western coast of
Europe into the interior of the continent, but the barometric minima
rise even more ; so that, for example, in Nertschinsk, in Eastern
Siberia, the mean of the barometric minima of January, reduced to the
sea-level, is 30·23 inches, and the lowest pressure happening in seven-
teen years in this month was equal to 29·93. If we consider that at this
place the temperature never rises above 14° F. in January, the effect
of cold and small quantity of vapour in the air in arresting the progress
of storms in winter will be clearly seen.   In European Russia the
barometric minima are lower in winter than in the other seasons.
This shows that the storms of the Atlantic take their course over
our country.   Speaking generally, the path of storms is from N.W.
to S.E. in winter, because they cannot advance in an eastward di-
rection as they began, being arrested by the cold.   The colder the
temperature is, the sooner the storms must turn to the southward :
and this will be much more the case in January than in November
and March, when the storms of Europe sometimes advance into the
interior of Siberia.

Another feature of the snow is that of arresting the progress of
temperature above the freezing-point so long as it lies.   In rising
above, the heat is employed in melting the snow ; or, in the language
of the mechanical theory of heat, it is transformed into work.   We
have some striking facts of this kind in Russia.   For example, Bar-
naul, in Western Siberia, has a winter temperature lower than St.
Petersburg by nearly 18° F. Yet the thermometer sometimes rises as
high as in this last place in winter, because Barnaul has the Kirghi-
steppes to the south-west.   As they are seldom covered with snow,
warm winds can pass across them and without losing their heat,
while before arriving at St. Petersburg they must lose much of their
heat in melting the snow over an extensive tract.   The result is that
seldom a winter month passes without temperatures above freezing-
point ; but in January and February the thermometer does not rise
above 39°, while at Barnaul a temperature of 42° may occur at that
time (for example, on the 4th, 5th, and 6th of February 1855).

I have mentioned already the effect of the snow in checking the
rise of temperature, and employing more abundant heat in melting.
This is most felt in spring, and lowers much the temperature of this
time of the year—as, for example, while in Central Europe, at some
distance from the sea, April has nearly the same temperature as October
in the same latitude, in Russia the warmth of the sun's rays cannot
raise the temperature of the air so much, and April is generally 4° F.
colder than October, while May has the same temperature as Sep-
tember.   As soon as the snow is melted our climate assumes its
true continental character.   In more northern parts of Russia it is
May which stays behind September—as, for example, at Archangel,
Berezoe, and even Yakutsk in one of the most continental cli-
mates of our planet : in this last place May is more than 3° colder
than September, while March is $13\frac{1}{2}°$ *warmer* than November.

I must now state a last point—the influence of forests in equalizing the layer of snow and giving to it all its beneficial effects. Without the forests a great mass of snow is often a check to all communication, as, for example, at this moment in South Russia, where most of the railways are stopped. The unusually great mass of snow is blown in all directions by the wind unimpeded by trees, as some of these places were always steppes; in others man was too short-sighted to let the trees stand. The effect of the melting of snow on the rising of rivers will be quite different in a wooded and a bare country. In the first the snow will lie sometimes a month longer than in the last, and accordingly the floods of the rivers will be longer continued but less high and devastating. Every one who has inhabited the country will be struck by this fact, and its bearing on the climate and the well-being of the population all around. Generally speaking, as I have stated, the effects of a layer of snow are beneficent to man. The proportion of the crops is of enormous economical worth. The greater moisture of the air is also good; and even the cold of spring, caused by the melting of snow, has its good side. The too rapid advance of vegetation in early spring is checked by it, and protracted to a time when the vegetables have less to fear from night frosts. Northern Europe, for example, suffers much less from this curse than the south, where the returns of cold in spring cause great damage every year. Only two serious effects are sometimes felt—the interruption of communication in snow-storms, and the great floods of spring. But both of these drawbacks can be avoided by the foresight of man, as forests arrest the progress of winds and cause a slow melting in spring, so as to store a great quantity of water to supply our rivers.—Silliman's *American Journal,* July 1871.

ON ELECTRO-TELEGRAPHY.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

<div align="right">Telegraph Street,
July 15, 1871.</div>

As M. Schwendler has referred to a statement of mine in his paper in your July Number, may I be allowed to say he is correct in stating that the " shunt " he describes was invented by a telegraph clerk (Mr. Higgins). Its application to overland telegraphs is mentioned in the handbooks supplied to the staff of the Indian Telegraph Department in 1867, where it is also stated that the proper resistance of the " shunt " is about equal to that of the electromagnet to which it is attached.

<div align="center">I am, Sir,
Your obedient Servant,
R. S. Culley.</div>

ON THE ERUPTION THEORY OF THE CORONA.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In your Supplementary Number for June last you say (page 537) that " Mr. Proctor has lately broached a theory of solar eruption, in which he considers that the solar coronal matter consists of meteors *ejected* from the sun, and rushing through the photosphere with a velocity of 200 miles per second," &c.

I presume that Mr. Proctor will be as much surprised as myself at this affiliation, inasmuch as in Fraser's Magazine for April last he discussed this idea as one which " astronomers first saw enunciated in Mattieu Williams's ' Fuel of the Sun,' " and pointed out the confirmations which the recent observations on the corona, and the actual measurements of eruptive velocities by Zöllner, afforded to this explanation, which incidentally and unexpectedly forced itself upon me in working out the necessary physical consequences of the unlimited extension of ordinary atmospheric matter in the work to which Mr. Proctor referred.

As the Philosophical Magazine is one of the permanent records of the progress of science, it is but a matter of justice both to Mr. Proctor and myself that this mistake should be corrected.

<div align="right">

Yours truly,

W. Mattieu Williams.
</div>

Woodside Green, Croydon,
    June 29, 1871.

[Although to a certain extent a similarity may exist between the theory enunciated by Mattieu Williams in his work entitled the ' Fuel of the Sun ' and that which we have ascribed to Proctor, in some important particulars they differ materially. It is generally understood that the principal material of the solar prominences is glowing hydrogen, which, according to Proctor, rushes outwards through the photosphere in countless exceedingly fine jets. According to Williams, the elements of water and metallic vapours are ejected by continuous explosions from the body of the sun during the uprush necessary to restore the equilibrium disturbed by the downrush forming the spots. Beyond the photosphere, the products of combustion of metallic elements solidify in consequence of a series of condensations, by which metallic snow and hail are produced, filling for a time the coronal space and then are rained back upon the sun. The only point, so far as we can see, in which the two theories agree is that the corona consists of solar matter; but as to the nature of the materials the theorists differ.—Reviewer.]

XX. *On the Reduction of the Second Axiom of the Mechanical Theory of Heat to general Mechanical Principles.* By R. CLAUSIUS[*].

1. IN a memoir recently communicated and published[†], I have advanced the following theorem, valid for every stationary motion of any system of material points :—*The mean vis viva of the system is equal to its virial.* This theorem may be regarded as one of dynamical equilibrium, since it gives a relation which must subsist between the forces and the motions called forth by them in order that the latter may continue with their *vis viva,* on the average, neither increased by positive work of the forces nor diminished by negative, but, amid passing fluctuations, maintaining a constant mean value.

As the magnitude which I have denoted by the name *virial* is, with equal coordinates of the material points, proportional to the forces operating upon them, the *vis viva* of stationary motion is, *cæteris paribus,* proportional to the forces which it balances. If, then, we regard heat as a stationary motion of the smallest particles of bodies, and absolute temperature as the measure of the *vis viva,* we shall find no difficulty in recognizing the agreement of the above-mentioned mechanical theorem with the law advanced by me in an earlier memoir[‡] :—*The effective force of heat is proportional to the absolute temperature.*

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[*] Translated from a separate impression communicated by the Author, having been read before the Niederrheinische Gesellschaft für Natur- und Heilkunde, on November 7, 1870.

[†] *Sitzungsberichte der Niederrheinischen Gesellschaft f. Nat. u. Heilk.* June 1870; Pogg. *Ann.* vol. cxli. p. 124; Phil. Mag. S. 4. vol. xl. p. 122.

[‡] Pogg. *Ann.* vol. cxvi. p. 73; *Abhandlungen über die mechanischen Wärmetheorie,* vol. i. p. 242.

If, however, we wish to make this law the basis of a mathematical development, we must give it a more definite form, because the expression *effective force of heat* may admit of different interpretations.   Hence in that memoir I have, for the purpose of thus applying it, expressed the law more fully, as follows:—

The mechanical work which can be done by heat in any alteration of the arrangement of a body, is proportional to the absolute temperature at which the alteration takes place.

In order to express this law by a mathematical equation, let us imagine the body undergoing an infinitely small alteration of its condition, the change proceeding in a reversible manner, in which the quantity of heat contained in the body as well as its constituents may be altered.   Work may either be performed (when the internal and external forces operating on the particles are overcome) or expended (when the particles yield to the forces). This infinitesimal work may be denoted by $d\mathrm{L}$; work performed is reckoned as positive, and work expended as negative.   Then the following equation will stand as the expression of the above law :—

$$d\mathrm{L} = \frac{\mathrm{T}}{\mathrm{A}}\, d\mathrm{Z}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

in which T denotes the absolute temperature, and A a constant, namely the caloric equivalent of the work, and Z represents a magnitude which is perfectly determined by the present condition of the body, without it being necessary to know in what way the body has come into this condition.   This magnitude I have named the *disgregation* of the body.

If we further assume, as I have done in the above-mentioned memoir, that the absolute temperature of a body is proportional to the quantity of heat present in it, and denote this quantity by H, we can put

$$\mathrm{T} = \mathrm{CH},$$

in which C will be a constant.   The preceding equation is thus transformed into

$$d\mathrm{L} = \frac{\mathrm{CH}}{\mathrm{A}}\, d\mathrm{Z}.$$

The fraction herein occurring, $\dfrac{\mathrm{H}}{\mathrm{A}}$, represents the quantity of heat present in the body, measured, not according to the usual heat-scale, but mechanically; therefore, in other words, it represents the *vis viva* of that motion which we name heat.   By introducing for this magnitude the simple sign h, the equation becomes

$$d\mathrm{L} = \mathrm{C}h\, d\mathrm{Z}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

We have now to find for this equation an explanation founded on mechanical principles. For this purpose the above theorem concerning the virial furnishes a clue, inasmuch as it indicates the nature of the considerations which must be employed. But it is not alone sufficient; the investigation requires in addition certain new and peculiar developments, which are to form the subject of the present memoir.

2. To begin with a case as simple as possible in relation to the kind of motion, and thereby facilitate the view of the mode of consideration which here comes into use, we will first suppose a single material point, operated on by a force which may be represented by an ergal—that is, the components of which, referred to three rectangular-coordinate directions, are expressed by the partial differential coefficients of the three coordinates of the point, taken negatively. Under the influence of this force, the point will have a periodical motion in a closed path.

Now let us imagine this motion to undergo an infinitely small alteration, resulting in a new periodical motion. This conversion of the motion can be occasioned in three ways : at any place in the path, through a passing external influence, the velocity-components $\frac{dx}{dt}$, $\frac{dy}{dt}$, and $\frac{dz}{dt}$ may be infinitesimally altered, and then the point may again be left to the operation merely of the original force; or an infinitesimal alteration may occur in the force operating on the point—for example, a change in the value of a constant occurring in the ergal. The third cause of conversion of the motion will not occur in our considerations on heat, but is of interest for a comparison which we shall make further on : it is the point being compelled to describe a path somewhat deviating from the one chosen by itself—which is also connected with an alteration of the force, because then to the original force is added the resistance which the new path-curve has to perform.

We will now investigate whether, in all these circumstances, there exists a universally valid relation between the alterations of the different magnitudes occurring in the motion.

3. The alterations undergone by the coordinates of the point, its velocity-components, the components of the force, &c. shall, as *differentials* of those magnitudes, be denoted as usual by the prefix d; so that, for example, dx will signify the variation in x during the time dt. On the other hand, the alterations of those magnitudes which result from a different motion taking the place of the original one shall be called *variations* of the magnitudes, and be denoted by prefixing the letter δ; so that, *e. g.*, the difference between a value of x in the original motion and the corresponding value in the altered motion will be signified by δx.

In reference to the latter, however, a special remark must be made, which is of importance for the following. If the altered motion is to be compared with the initial one in such a manner as to show how the values of x in the one differ from the corresponding values of x in the other motion, we must first settle which values of x shall be regarded as corresponding to each other. For this purpose, any two points infinitely near each other in the two paths may first be taken as corresponding points. Starting from these, in order to obtain the remaining corresponding points we take as a measure a magnitude which changes in the course of the motions, and settle that those points in the two paths which belong to equal values of the measuring magnitude are corresponding points. As measuring magnitude, however, one must be chosen which for an entire revolution has equal values in both paths; for through an entire revolution the moving point always arrives again at the chosen initial point in each of the two paths, and these we have already taken as corresponding points.

We will now determine the measuring magnitude in the following manner. Let i be the time of a revolution with the original motion, and t the variable time which the moveable point requires in order to pass from the initial position to another one; then we will put

$$t = i \cdot \phi. \quad \ldots \quad \ldots \quad \ldots \quad (3)$$

For the altered motion, let the time of a revolution be denoted by i', and the variable time, reckoned from the point's leaving its initial position, by t'; then we put

$$t' = i' \cdot \phi.$$

If, now, ϕ has equal values in both expressions, t and t' are corresponding times. The corresponding times being in this manner determined, the corresponding points of the two paths and, accordingly, the corresponding values of x, y, z, &c. follow of themselves.

The magnitude ϕ we will call the phase of the motion. During one revolution the phase increases one unit. With further increase, the phases which differ by a whole number of units may be regarded as equal, in the same sense as angles which differ by multiples of 2π.

Subtracting the first of the two preceding equations from the second, there results

$$t' - t = (i' - i)\phi.$$

The difference $t' - t$ is the variation of t, and the difference $i' - i$ the variation of i. Denoting these by δt and δi, we can write

$$\delta t = \delta i \cdot \phi, \quad \ldots \quad \ldots \quad \ldots \quad (4)$$

whence it follows as a rule that, if we wish to *variate* equation (3), we must regard the magnitude ϕ as constant. On the contrary, if we wish to *differentiate* the same equation, we must regard i as constant, because the differentiation refers to the course of a determinate motion, in which the time of a revolution i is a given magnitude. We thus obtain

$$dt = i\,d\phi. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

4. These preliminaries being settled, we can now proceed to the proposed mathematical development. Taking the expression $\dfrac{dx}{dt}\delta x$, and differentiating it according to ϕ, we obtain

$$\frac{d}{d\phi}\left(\frac{dx}{dt}\delta x\right) = \frac{d^2x}{dt\,d\phi}\delta x + \frac{dx}{dt}\cdot\frac{d(\delta x)}{d\phi}. \quad . \quad . \quad . \quad (6)$$

Now, as in variation the phase ϕ is regarded as constant, we can, when a magnitude varies and is to be differentiated according to ϕ, change the order of these two operations and therefore put

$$\frac{d(\delta x)}{d\phi} = \delta\frac{dx}{d\phi}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Thereby the preceding equation changes into

$$\frac{d}{d\phi}\left(\frac{dx}{dt}\delta x\right) = \frac{d^2x}{dt\,d\phi}\delta x + \frac{dx}{dt}\delta\frac{dx}{d\phi}. \quad . \quad . \quad . \quad (8)$$

This equation may be transformed in the following manner :—

$$\begin{aligned}
\frac{d}{d\phi}\left(\frac{dx}{dt}\delta x\right) &= \frac{d^2x}{dt^2}\cdot\frac{dt}{d\phi}\delta x + \frac{dx}{dt}\delta\left(\frac{dx}{dt}\cdot\frac{dt}{d\phi}\right)\\
&= \frac{d^2x}{dt^2}\cdot\frac{dt}{d\phi}\delta x + \frac{dx}{dt}\cdot\frac{dt}{d\phi}\delta\frac{dx}{dt} + \left(\frac{dx}{dt}\right)^2\delta\frac{dt}{d\phi}\\
&= \frac{d^2x}{dt^2}\cdot\frac{dt}{d\phi}\delta x + \frac{1}{2}\frac{dt}{d\phi}\delta\left(\frac{dx}{dt}\right)^2 + \left(\frac{dx}{dt}\right)^2\delta\frac{dt}{d\phi}.
\end{aligned}$$

Putting herein, for the differential coefficient $\dfrac{dt}{d\phi}$, its value from equation (5), there results

$$\frac{d}{d\phi}\left(\frac{dx}{dt}\delta x\right) = i\frac{d^2x}{dt^2}\delta x + \frac{1}{2}i\delta\left(\frac{dx}{dt}\right)^2 + \left(\frac{dx}{dt}\right)^2\delta i. \quad . \quad (9)$$

This equation shall now be multiplied by $d\phi$ and then integrated from $\phi = 0$ to $\phi = 1$; that is, for an entire revolution.

The integration on the left-hand side may proceed at once, and we obtain

$$\int_0^1 \frac{d}{d\phi}\left(\frac{dx}{dt}\delta x\right)d\phi = \left(\frac{dx}{dt}\delta x\right)_1 - \left(\frac{dx}{dt}\delta x\right)_0,$$

in which $\left(\dfrac{dx}{dt}\delta x\right)_0$ and $\left(\dfrac{dx}{dt}\delta x\right)_1$ signify the initial and the final value of $\dfrac{dx}{dt}\delta x$.

As with an entire revolution the final is equal to the initial value, the equation passes into

$$\int_0^1 \frac{d}{d\phi}\left(\frac{dx}{dt}\,\delta x\right)d\phi = 0. \quad . \quad . \quad . \quad . \quad (10)$$

As to the terms on the right-hand side, it is first to be remarked that in the integration according to ϕ the magnitudes i and δi are to be regarded as constant. Further, when any magnitude dependent on ϕ is to be integrated from 0 to 1 (for example, x), the following equation can be formed :—

$$\int_0^1 x\,d\phi = \frac{1}{i}\int_0^1 x\,dt.$$

Distinguishing the mean value from the variable quantity by putting a horizontal stroke over the sign which represents the variable, we can write

$$\int_0^1 x\,d\phi = \bar{x}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

What is here said of the quantity x holds good also of the quantities $\dfrac{d^2x}{dt^2}\delta x$, $\left(\dfrac{dx}{dt}\right)^2$, and $\delta\left(\dfrac{dx}{dt}\right)^2$, occurring on the right-hand side of the above equation. In reference to the last quantity it is further to be remarked that the mean value of a variation is equal to the variation of the mean value—that thus we can write

$$\overline{\delta\left(\frac{dx}{dt}\right)^2} = \delta\overline{\left(\frac{dx}{dt}\right)^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Accordingly the equation obtained by integrating equation (9) is the following,

$$0 = i\,\overline{\frac{d^2x}{dt^2}\,\delta x} + \tfrac{1}{2}i\delta\overline{\left(\frac{dx}{dt}\right)^2} + \overline{\left(\frac{dx}{dt}\right)^2}\delta i, \quad . \quad . \quad . \quad (13)$$

or, dividing by i, and transposing the first term on the right-hand side to the left,

$$-\overline{\frac{d^2x}{dt^2}\,\delta x} = \tfrac{1}{2}\,\delta\overline{\left(\frac{dx}{dt}\right)^2} + \overline{\left(\frac{dx}{dt}\right)^2}\,\delta\log i. \quad . \quad . \quad (14)$$

Precisely similar equations to those here derived for the x co-

ordinate are valid also for the y and z coordinates, viz. :—

$$-\overline{\frac{d^2y}{dt^2}\,\delta y} = \tfrac{1}{2}\,\delta\overline{\left(\frac{dy}{dt}\right)^2} + \overline{\left(\frac{dy}{dt}\right)^2}\delta\log i, \quad . \quad . \quad (14a)$$

$$-\overline{\frac{d^2z}{dt^2}\,\delta z} = \tfrac{1}{2}\,\delta\overline{\left(\frac{dz}{dt}\right)^2} + \overline{\left(\frac{dz}{dt}\right)^2}\delta\log i. \quad . \quad . \quad (14b)$$

Adding these three equations, and at the same time taking into consideration that

$$\left(\frac{dx}{dt}\right)^2 + \left(\frac{dy}{dt}\right)^2 + \left(\frac{dz}{dt}\right)^2 = v^2, \quad . \quad . \quad . \quad (15)$$

in which v signifies the velocity of the point, the result is :—

$$-\overline{\left(\frac{d^2x}{dt^2}\,\delta x + \frac{d^2y}{dt^2}\,\delta y + \frac{d^2z}{dt^2}\,\delta z\right)} = \tfrac{1}{2}\delta\overline{v^2} + \overline{v^2}\delta\log i. \ (16)$$

If we multiply this equation by the mass m of the material point, we can introduce, instead of the products $m\dfrac{d^2x}{dt^2}$, $m\dfrac{d^2y}{dt^2}$, and $m\dfrac{d^2z}{dt^2}$, the three components (taken in the directions of the coordinates) of the force operating on the point, which may be denoted by X, Y, and Z, thus :—

$$-\overline{(\mathrm{X}\delta x + \mathrm{Y}\delta y + \mathrm{Z}\delta z)} = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\delta\log i. \quad . \quad (17)$$

In reference to the force operating on the point, we have pre-supposed that its three components may be represented by the partial differential coefficients, taken negatively, of a function of the coordinates of the point. If, for the original motion, we denote this function (which we call the ergal of the point) by U, we can give to the preceding equation the following form :—

$$\overline{\frac{d\,\mathrm{U}}{dx}\,\delta x + \frac{d\,\mathrm{U}}{dy}\,\delta y + \frac{d\,\mathrm{U}}{dz}\,\delta z} = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\delta\log i, \quad . \quad . \quad (18)$$

or, more briefly,

$$\delta\overline{\mathrm{U}} = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\delta\log i. \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

5. In this equation we must first consider the expression $\delta\overline{\mathrm{U}}$.

In every case in which, with the altered motion, the ergal is still represented by the same function U as with the original the quantity $\delta\overline{\mathrm{U}}$ (the alteration of the mean value of the ergal) expresses the work done in the transition from the one stationary motion to the other. If, then, as we have done above in the

equations referring to heat, we denote by δL the work performed, we can put

$$\delta L = \delta \overline{U}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

When, on the other hand, the change in the motion is occa- sioned by the fact that the force operating on the point has been changed, the thing is not quite so simple, but requires special consideration.

6. As said above, the alteration of the force may be imagined, mathematically, conditioned by a constant which occurs in the ergal changing its value by an infinitely small quantity. With- out going into this more closely, we will only make the following assumption, which comes to essentially the same thing. The ergal, which with the original motion was represented by the function U, shall with the altered motion be represented by the sum $U + \mu V$, in which V signifies any other function of the co- ordinates, and μ an infinitely small constant factor.

In regard to the occurrence of the increase μV, however, we will preliminarily make the subsidiary assumption that the in- crease does not take place suddenly at a certain moment, but proceeds gradually, during an entire revolution,—the infinitely small factor which stands before V increasing uniformly during that time, so as just to reach the value μ at the end of the revo- lution, and then preserving this value constant during the suc- ceeding revolutions. Accordingly, during one element of the time dt, the factor will increase by $\dfrac{\mu \, dt}{i}$, or, which is the same thing, during an element of the phase $d\phi$ the factor will increase by $\mu \, d\phi$.

In order now to determine the work-variation δL which cor- responds to the entire transition from the one stationary motion to the other, we must first give the work-variation for any selected individual phase ϕ_1. For this purpose let us consider the move- able point from the moment when, in its revolution in the ori- ginal path, it just passes the place which belongs to the phase ϕ_1, and let us follow it hence through two entire revolutions. These comprise, 1st, the remainder of the revolution commenced in the original path; 2nd, the revolution during which the alte- ration in the ergal takes place; and, 3rd, the commencement of the revolution in the new path as far as the phase ϕ_1. The work done during this time may be divided into two quantities, corre- sponding to the original ergal U and the increase μV.

The first quantity is expressed very simply; for if U_1 denotes the value of U in the original path belonging to the phase ϕ_1, and $U_1 + \delta U_1$ the value belonging to the same phase in the new path, then δU_1 is the first quantity of work.

In the determination of the second quantity, we must, on account of the gradual nature of the increase μV, conceive the factor μ divided into an infinite number of parts, and for each part reckon as the initial value of V that which corresponds to the place where the moveable point was at the moment when this part commenced. Considering thus the part $\mu d\phi$, which has arisen during the phase-element from ϕ to $\phi + d\phi$, we have to form for it as expression of the work the difference

$$\mu d\phi (V_1 - V),$$

in which V and V_1 denote those function-values which belong to the phases ϕ and ϕ_1. Properly the variations of the function-values would also have to be taken into account, because the moveable point is, from the beginning of alteration of the force, no longer in the original path. As, however, these variations are infinitesimal and the factor μ is also infinitely small, only infinitely small quantities of a higher order would hence arise, which may be neglected. In order, then, to extend to the whole increase the above expression, which is valid for an infinitesimal part of it μV, we must integrate it from 0 to 1. By resolving the parenthesis the expression is divided into two terms. The first gives $\mu V_1 d\phi$ by integration, or, since V_1 is independent of ϕ, simply μV_1. The integral of the other term, $\mu V d\phi$, can be represented by $\mu \overline{V}$, if $\overline{V}$ denote the mean value of V during an entire revolution. Accordingly the second quantity sought is

$$\mu (V_1 - \overline{V}).$$

By adding the two quantities, we obtain the variation of work corresponding to the phase ϕ_1, namely

$$\delta U_1 + \mu (V_1 - \overline{V}).$$

In order to deduce, further, the work δL, which refers to the whole alteration of the stationary motion, we must multiply this expression by $d\phi_1$, and once more integrate it from 0 to 1. We thus obtain

$$\delta L = \int_0^1 \delta U_1 d\phi_1 + \mu \int_0^1 (V_1 - \overline{V}) d\phi_1,$$

for which, since in the first term on the right-hand side the integral of the variation may be replaced by the variation of the integral, we may write

$$\delta L = \delta \int_0^1 U_1 d\phi_1 + \mu \int_0^1 (V_1 - \overline{V}) d\phi_1.$$

The integrals $\int_0^1 U_1 d\phi_1$ and $\mu \int_0^1 V_1 d\phi_1$ signify the mean values of

U_1 and V_1 during one revolution, or, which amounts to the same, the mean values of U and V during one revolution, which are denoted by $\overline{U}$ and $\overline{V}$. The integral $\int_0^1 \overline{V} d\phi_1$ is likewise equal to $\overline{V}$; and consequently

$$\delta L = \delta \overline{U} + \mu(\overline{V} - \overline{V}) = \delta \overline{U}.$$

We have thus for this case also arrived at the same simple result which we have already expressed for the other cases in equation (20)..

To obtain this result, we have made the special assumption that the alteration of the ergal proceeds uniformly during one entire revolution. But we may also extend this result to another case, and one which is important for the following. We will imagine that, instead of one point in motion, there are several, the motions of which take place in essentially like circumstances, but with different phases. If, now, at any time t the infinitely small alteration of the ergal occurs which is expressed mathematically as U changing into $U + \mu V$, we have for each single point, instead of $\mu(\overline{V} - \overline{V})$, to construct a quantity of the form $\mu(\overline{V} - V)$, in which V represents the value of the second function corresponding to the time t. This quantity is in general not $= 0$, but has a positive or negative value, according to the phase in which the point in question was at the time t. But if we wish to form the mean value of the quantity $\mu(\overline{V} - V)$ for all the points, we have, instead of the individual values which occur of V, to put the mean value $\overline{V}$, and thereby obtain again the expression $\mu(\overline{V} - \overline{V})$, which is $= 0$.

7. From the preceding it follows that, on the suppositions made, we can put δL in equation (19) in the place of δU, so that the equation becomes

$$\delta L = \frac{m}{2} \delta \overline{v^2} + m \overline{v^2} \delta \log i. \quad . \quad . \quad . \quad (21)$$

The expression on the right may be simplified by introducing h for the product $\frac{m}{2} \overline{v^2}$, which represents the mean *vis viva* of the point. Thence comes

$$\delta L = \delta h + 2h \delta \log i. \quad . \quad . \quad . \quad . \quad (22)$$

By the help of this equation we can determine the mechanical work which is done in the change from one stationary motion to another, differing infinitely little from it, without perfectly knowing the motions, since to take into account the mean *vis viva* and the time of a revolution is sufficient.

The expression containing the quantities h and i, which represents the work δL, is not a complete variation of a function of h and i; on the other hand, if the equation be brought into the following form,

$$\delta L = h\left(\frac{\delta h}{h} + 2\delta \log i\right)$$
$$= h(\delta \log h + 2\delta \log i),$$

the two variations in the brackets can be reduced to one, viz.

$$\delta L = h\delta \ (\log h + 2 \log i),$$

or, otherwise written,

$$\delta L = h\delta \log (hi^2). \quad . \quad . \quad . \quad . \quad . \quad (23)$$

Hence the work can be represented by the product of h and the variation of a function of h and i.

This result corresponds perfectly with equation (2) relative to the theory of heat,

$$dL = ChdZ.$$

The quantity $\log (hi^2)$ is replaced in this equation by the product CZ, in which C is a constant, and Z the magnitude which in the theory of heat I have named the disgregation. We have hence, so far as we wish to apply this conception to the stationary motion of a single point, arrived at a nearer determination of it—namely, that the disgregation is proportional to the quantity $\log (hi^2)$.

8. In order to get an idea of the geometrical meaning of the quantity $\log (hi^2)$, I will for h reintroduce the product $\frac{m}{2} \overline{v^2}$. We then obtain

$$\log (hi^2) = \log \left(\frac{m}{2} \overline{v^2}. \ i^2\right)$$
$$= \log (\overline{v^2}. \ i^2) + \log \frac{m}{2}$$
$$= 2 \log (i \sqrt{\overline{v^2}}) + \log \frac{m}{2}.$$

The last term on the right-hand side is invariable, and hence is unimportant to equation (23), in which only the variation of the quantity considered occurs; we need therefore only attend to the first term.

Assuming now as a special case that the velocity is constant (which occurs, for example, when a point moves in a circular path round a fixed centre of attraction, or when a point operated on by no other force flies forward and backward between fixed

clastic walls, from which it rebounds with equal velocity at each impact), we can write simply v^2 for $\overline{v^2}$, and then extract the root, by which the expression $i\sqrt{\overline{v^2}}$ is changed into iv. This product is equal to the length of the point's path; and consequently we can say that, in motions with constant velocity, the disgregation (neglecting an additive constant, which in the variation or differentiation is omitted) *is proportional to the logarithm of the length of the path.*

When the velocity is variable, the thing is not quite so simple, because the mean value of the square of the velocity is different from the square of its mean value; but still it is seen that the disgregation stands in close relation to the logarithm of the length of the path.

9. Before leaving the motion of a single point in order to pass to more extended investigations, it will be to the purpose to submit to a further special consideration the last of the three causes above mentioned of the conversion of the motion, because we shall thereby have an opportunity of comparing the result of our development with a well-known and important mechanical theorem.

We will, namely, assume the conversion to be occasioned by the point having been compelled, instead of the path chosen by itself, to describe another, lying infinitely near it. In this case, for each place in the changed path, compared with the corresponding place in the original path, according to the theorem of the equivalence of *vis viva* and mechanical work, the following equation holds good :—

$$\delta U + \frac{m}{2}\delta(v^2) = 0.$$

Accordingly, in equation (19), instead of $\delta\overline{U}$, we can put $-\frac{m}{2}\delta\overline{v^2}$, and hence obtain the following equation :—

$$-\frac{m}{2}\delta\overline{v^2} = \frac{m}{2}\delta\overline{v^2} + m\overline{v^2}\delta\log i,$$

from which, by easy transformations, result :—

$$m\delta\overline{v^2} + m\overline{v^2}\frac{\delta i}{i} = 0,$$

$$i\delta\overline{v^2} + \overline{v^2}\delta i = 0,$$

$$\delta(\overline{v^2}\cdot i) = 0,$$

$$\delta\int_0^1 v^2 dt = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

This equation is in form the same as that which, for a single

moveable point, expresses the *theorem of the least effect.* It is true that in the signification there is a difference, inasmuch as in deducing our equation we have supposed that the original and the altered motion take place in closed paths which need not coincide in any point, while in the theory of the least effect it is supposed that both motions begin from a common point and end at a common point; yet this difference is immaterial to the proof, because equation (24) can be deduced equally on both suppositions, if in the one case we understand by i the time of a revolution, and in the other that time which the moving point requires in order to pass from the initial to the final position.

Returning now to our more general result, expressed by equation (23), on comparing it with the theorem of the least effect, its applicability is seen to be more extended, inasmuch as it includes also the cases in which the *vis viva* is altered by a transient extraneous influence, or into which a change in the ergal enters, whereas such cases are excluded from the theorem of the least effect *.

10. Having treated the simple case of a single point moving in a closed path, let us now pass to more complicated ones.

We will assume that there are a very great number of material points which, on the one hand, exercise forces upon each other, and, on the other, are affected by forces from without. Under the influence of all these forces the points shall move in a sta-

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* It may, in passing, be further remarked that where the forces present consist of central forces proportional to a definite (positive or negative) power of the distance, the equations here developed are capable of being combined very simply with the equation which expresses the theorem of the virial. That is to say, in such cases the virial differs from the mean value of the ergal only by a constant factor; for when a force denoted generally by $\phi(r)$ is determined by the equation

$$\phi(r) = kr^n,$$

in which k and n are constants, we obtain by integration, if we suppose the arbitrary constant equal to 0,

$$\int \phi(r) dr = \frac{k}{n+1} r^{n+1};$$

and accordingly the equation

$$\tfrac{1}{2} r \phi(r) = \frac{n+1}{2} \int \phi(r) dr$$

is valid; and hence the virial is equal to the mean value of the ergal multiplied by the factor $\frac{n+1}{2}$. Consequently the theorem of the virial can for such cases be expressed thus :—*The mean* vis viva *is equal to the mean ergal multiplied by* $\frac{n+1}{2}$. It is obvious how all equations which contain the mean *vis viva* and the mean ergal can be simplified by the application of this theorem.

tionary manner. We will presuppose that the forces have an ergal—that is, that the work which is done by the whole of the forces in an infinitely small change in the situation of the points is expressed by the negative differential of a function of the whole of the coordinates. When the original stationary motion is converted into another stationary motion, the forces shall still have an ergal, which, however, may differ from the preceding not merely by the altered situation of the points, but also by another circumstance. This circumstance may be conceived to be mathematically expressed by the ergal containing a quantity which is constant during each stationary motion, but alters its value from one stationary motion to the other.

Further, we will make a supposition which will facilitate our further considerations, and corresponds to what takes place in the motion which we name heat. If the body the heat-motion of which is in question is chemically simple, all its atoms are equal to one another; but if it is a chemical compound, there are indeed different kinds of atoms, but the number of each kind is very great. Now all these atoms are not necessarily found in like circumstances. When, for instance, the body consists of parts in different states of aggregation, the atoms belonging to one part move differently from those belonging to the other. Yet we can still assume that each kind of motion is carried out by a very great number of equal atoms essentially under equal forces and in like manner, so that only the synchronous phases of their motions are different. In correspondence with this we will now presume also that, in our system of material points, different kinds of them may occur, but of each kind a very great number are present, and also that the forces and motions are such that at all times a great number of points, under the influence of equal forces, move equally, and only have different phases.

Lastly, we will, for the sake of simplicity, make one more assumption, which will afterwards be dropped again, namely that all the points describe closed paths. For such points as have been said above to move alike, we just now make a special assumption—that they describe equal paths with equal times of revolution, while other points may describe other paths with other times of revolution. When the original is changed into another stationary motion, the paths and the times of revolution are altered, but again only closed paths with fixed times of revolution shall occur, of which each holds good for a great number of points.

11. On these conditions let us now again consider the product $\frac{dx}{dt}\delta x$ for any point, or (at once multiplying it by the mass

m of the point) the product $m \frac{dx}{dt} \delta x$, in which δx, as before, signifies the difference between a value of x in the original path and the value of x at the corresponding place in the altered path.

This product changes its value periodically during the motion of the point, so that at the expiration of every time of revolution i it returns to its former value. We can hence construct the following equation :—

$$\int_0^1 \frac{d}{dt} \left(m \frac{dx}{dt} \delta x \right) dt = 0.$$

This equation can be further simplified when we consider not merely one material point, but an entire group which move equally and hence have the same time of revolution i. The quantity $m \frac{dx}{dt} \delta x$ changes its value according to the phase in which the point is. As, however, at a fixed time the points belonging to the group have different phases, and the number of the points constituting the group is so great that at every time all the phases may be considered to be proportionately represented, the value of the sum

$$\Sigma m \frac{dx}{dt} \delta x,$$

referred to all these points, will not perceptibly vary. The same holds good for every other group of points of like kind and with equal motion ; and hence we can at once refer the preceding sum to all the points of our system and likewise regard as constant the sum so completed. We thus obtain the equation

$$\frac{d}{dt} \Sigma m \frac{dx}{dt} \delta x = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

We will now carry out the differentiation herein indicated :

$$\frac{d}{dt} \Sigma m \frac{dx}{dt} \delta x = \Sigma m \frac{d^2x}{dt^2} \delta x + \Sigma m \frac{dx}{dt} \cdot \frac{d(\delta x)}{dt}. \quad . \quad (26)$$

In the expression $\frac{d(\delta x)}{dt}$, in which the quantity x is first variated, and then differentiated according to t, the order of these two operations must not be reversed. Probably, however, this may be done when the differentiation refers not to the time t, but to the phase ϕ. Hence we form the following equation,

$$\frac{d(\delta x)}{dt} = \frac{d(\delta x)}{d\phi} \cdot \frac{d\phi}{dt},$$

or by replacing (in accordance with equation 5) the differential

coefficient $\frac{d\phi}{dt}$ by the fraction $\frac{1}{i}$, the equation

$$\frac{d(\delta x)}{dt} = \frac{1}{i} \cdot \frac{d(\delta x)}{d\phi}.$$

Here we may exchange the differentiation and the variation on the right-hand side, whereby we obtain

$$\frac{d(\delta x)}{dt} = \frac{1}{i} \delta \frac{dx}{d\phi}.$$

After this exchange, we again introduce on the right-hand side the differential coefficient according to t, putting

$$\frac{dx}{d\phi} = \frac{dx}{dt} \cdot \frac{dt}{d\phi} = i \frac{dx}{dt}.$$

Thereby we obtain

$$\begin{aligned}
\frac{d(\delta x)}{dt} &= \frac{1}{i} \delta \left(i \frac{dx}{dt} \right) \\
&= \frac{1}{i} \left(i\delta \frac{dx}{dt} + \frac{dx}{dt} \delta i \right) \\
&= \delta \frac{dx}{dt} + \frac{dx}{dt} \delta \log i.
\end{aligned}$$

By employing this equation, equation (26) is changed into

$$\begin{aligned}
\frac{d}{dt} \Sigma m \frac{dx}{dt} \delta x &= \Sigma m \frac{d^2 x}{dt^2} \delta x + \Sigma m \frac{dx}{dt} \left(\delta \frac{dx}{dt} + \frac{dx}{dt} \delta \log i \right) \\
&= \Sigma m \frac{d^2 x}{dt^2} \delta x + \Sigma \frac{m}{2} \delta \left(\frac{dx}{dt} \right)^2 + \Sigma m \left(\frac{dx}{dt} \right)^2 \delta \log i. \quad (27)
\end{aligned}$$

As, in accordance with equation (25), the differential coefficient here standing on the left-hand side is equal to 0, we hence obtain

$$-\Sigma m \frac{d^2 x}{dt^2} \delta x = \Sigma \frac{m}{2} \delta \left(\frac{dx}{dt} \right)^2 + \Sigma m \left(\frac{dx}{dt} \right)^2 \delta \log i. \quad (28)$$

In like manner for the two other coordinates we can form the following equations :—

$$-\Sigma m \frac{d^2 y}{dt^2} \delta y = \Sigma \frac{m}{2} \delta \left(\frac{dy}{dt} \right)^2 + \Sigma m \left(\frac{dy}{dt} \right)^2 \delta \log i, \quad (28a)$$

$$-\Sigma m \frac{d^2 z}{dt^2} \delta z = \Sigma \frac{m}{2} \delta \left(\frac{dz}{dt} \right)^2 + \Sigma m \left(\frac{dz}{dt} \right)^2 \delta \log i. \quad (28b)$$

When we add together these three equations, and at the same time consider the equation

$$\left(\frac{dx}{dt} \right)^2 + \left(\frac{dy}{dt} \right)^2 + \left(\frac{dz}{dt} \right)^2 = v^2,$$

there results :—

$$-\Sigma m \left(\frac{d^2x}{dt^2} \, \delta x + \frac{d^2y}{dt^2} \, \delta y + \frac{d^2z}{dt^2} \, \delta z \right) = \Sigma \frac{m}{2} \delta(v^2) + \Sigma m v^2 \delta \log i. \quad (29)$$

In this equation we now replace the products $m\dfrac{d^2x}{dt^2}$, $m\dfrac{d^2y}{dt^2}$, $m\dfrac{d^2z}{dt^2}$ by the force-components X, Y, Z, whereby it is changed into

$$-\Sigma(X\delta x + Y\delta y + Z\delta z) = \Sigma \frac{m}{2} \delta(v^2) + \Sigma m v^2 \delta \log i. \quad . \quad (30)$$

The left-hand side of the equation, thus transformed, must be subjected to a closer consideration.

12. Since, according to hypothesis, the forces operating in the system have an ergal, in all cases in which, on the transition from one stationary motion to the other, the ergal undergoes only the change conditioned by the altered position of the points, the left-hand side of the preceding equation is simply the variation of the ergal, and, as such, represents the work done in the transition from one motion to the other, which we have denoted by δL. When, on the contrary, the ergal undergoes a further change, containing a quantity which, as above said, is constant in each stationary motion, but the value of which changes in the transition from one motion to the other, the special circumstances under which this happens must also be taken into consideration.

For a single moving material point, it follows from our previous considerations that the work δL depends on the phase in which the point is at the moment when the alteration of the ergal occurs. On the other hand, we have also further seen that with a great number of points which are in different phases, so that at the moment of the alteration of the ergal all the phases are simultaneously represented, that difference vanishes for the mean value relative to all the points, and that hence we may, as far as the mean value is concerned, regard as the expression of the work δL that variation of the ergal which the mere *change of position of the points* supposes.

Such a case is our present one, where in every kind of motion occurring we have to do with very many points in the most various phases ; hence we can replace the left-hand side of the above equation simply by δL, whereby we obtain

$$\delta L = \Sigma \frac{m}{2} \delta(v^2) + \Sigma m v^2 \delta \log i. \quad . \quad . \quad (31)$$

13. In the preceding derivation it was specially presupposed that all the points describe closed paths. We will now drop this

supposition and only retain the assumption that the motion is stationary.

Since with motions which need not be in closed paths the notion of time of revolution is, in the literal sense, no longer applicable, the question arises whether another, corresponding, can be put in its place.

For this purpose let us first consider only those components of the motions which refer to one determined direction—for example, the components in the x direction of our system of coordinates. We have then to do simply with motions alternately to the positive and the negative side; and if, in particular, in relation to elongation, velocity, and duration manifold varieties occur, there yet is in the notion of a stationary motion the prevalence of a certain uniformity, on the whole, in the way that the same states of motion are repeated. Accordingly it must be possible to exhibit a mean value for the intervals of time within which the repetitions take place with each group of points that are alike in their motions. If we denote this mean duration of a period of motion by i, we can unhesitatingly assume as valid also for the motion now considered equation (28), namely

$$-\Sigma m \frac{d^2x}{dt^2}\, \delta x = \Sigma \frac{m}{2}\, \delta\left(\frac{dx}{dt}\right)^2 + \Sigma m \left(\frac{dx}{dt}\right)^2 \delta \log i.$$

Corresponding equations can be formed also for the y and z directions; and in fact we will assume that the motions in the various directions of the coordinates so far agree that, in each group of points, we may ascribe a common value to the quantity $\delta \log i$ for all three coordinate-directions. By then treating the three equations so formed as we have equations (28), (28a), and (28b) we arrive again at equation (31) :—

$$\delta L = \Sigma \frac{m}{2}\, \delta(v^2) + \Sigma m v^2 \delta \log i.$$

14. In the further treatment of this equation a difficulty occurs, because in the different groups of points both the velocity v and the duration of a motion-period i may vary, and hence these two quantities occurring under the last sign of summation cannot at once be separated. Nevertheless if we take advantage of a near-lying supposition, the separation becomes possible, and we thereby arrive at a very simple form of the equation.

As the various material points belonging to our system reciprocally operate upon each other, the *vis viva* of one group of points cannot be altered while the *vis viva* of the others remains unaltered ; but the alteration of the one supposes the alteration of the others, because a certain equilibrium between ths *vires vivæ* of the various points must be restored before the new state can remain stationary. We will now suppose that, in the motion

which we call heat, equilibrium is always effected in this way :—
between the *vires vivæ* of the various points a fixed ratio subsists,
which is renewed with every alteration that occurs in the total
vis viva. The mean *vis viva* of each point may then be repre-
sented by a product of the form $mc\mathrm{T}$, in which m is the mass of
the point, and c another constant, determinate for each point,
while T denotes a variable quantity which is equal for all the
points. By the insertion of this product in the place of $\frac{m}{2}v^2$,
the preceding equation becomes

$$\delta \mathrm{L} = \Sigma mc\delta \mathrm{T} + \Sigma 2mc\mathrm{T}\delta \log i. \quad . \quad . \quad . \quad (32)$$

Herein the quantity T, as a common factor, can be omitted from
the second sum. We could also omit the variation δT from the
first sum; but it may be left under the sign of summation.
Hence comes

$$\delta \mathrm{L} = \Sigma mc\delta \mathrm{T} + \mathrm{T}\Sigma 2mc\delta \log i$$

$$= \mathrm{T}\left(\Sigma mc\frac{\delta \mathrm{T}}{\mathrm{T}} + \Sigma 2mc\delta \log i\right)$$

$$= \mathrm{T}(\Sigma mc\delta \log \mathrm{T} + \Sigma 2mc\delta \log i); \quad . \quad . \quad (33)$$

or, combining the two sums into one and putting the sign of va-
riation before the sign of summation,

$$\delta \mathrm{L} = \mathrm{T}\delta \mathrm{L}\Sigma mc (\log \mathrm{T} + 2 \log i),$$

for which, finally, we may write

$$\delta \mathrm{L} = \mathrm{T}\delta\Sigma mc \log (\mathrm{T}i^2). \quad . \quad . \quad . \quad . \quad (34)$$

15. This last equation, if by T we understand the absolute
temperature, agrees perfectly with equation (1) adduced for heat,

$$d\mathrm{L} = \frac{\mathrm{T}}{\mathrm{A}} d\mathrm{Z},$$

in order to explain it on mechanical principles. The *disgrega-
tion* of the body, Z, is according to this represented by the ex-
pression

$$\mathrm{A}\Sigma mc \log (\mathrm{T}i^2).$$

It is easy to show also its agreement with another equation of
the theory of heat.

Let us imagine *vis viva* communicated to our system of mate-
rial points by a transitory external influence, and the system then
left to itself; this *vis viva* may partly serve to increase the *vis
viva* present in the system, and partly be expended in mechanical
work. Hence, if δq denotes the *vis viva* communicated, and h

that present in the system, we can write:—

$$\delta q = \delta h + \delta L$$
$$\cdot = \delta \Sigma mcT + \delta L$$
$$= \Sigma mc \delta T + \delta L.$$

Replacing herein δL by its value from (33), we obtain

$$\delta q = \Sigma 2 mc \delta T + T \Sigma 2 mc \delta \log i$$
$$= T(\Sigma 2 mc \delta \log T + \Sigma 2 mc \, \delta \log i)$$
$$= T \Sigma 2 mc \delta \log (Ti),$$

or, otherwise written,

$$\delta q = T \delta \Sigma 2 mc \log (Ti). \quad . \quad . \quad . \quad (35)$$

This equation corresponds to equation (59) in my memoir "On some convenient Forms of the fundamental Equations of the Mechanical Theory of Heat"[*]. If, namely, we multiply both sides of the preceding equation by A (the caloric equivalent of the work), and then, for $A\delta q$ (which represents in heat-measure the communicated *vis viva*) put δQ, and introduce the quantity S with the signification

$$S = A \Sigma 2 mc \log (Ti), \quad . \quad . \quad . \quad . \quad (36)$$

the preceding equation is changed into

$$\delta Q = T \delta S. \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

The quantity S here is that which I have named the *entropy* of the body.

In the last equation the sign of variation may be replaced by the sign of differentiation, since, of the two processes previously considered together (the variation during a stationary motion, and the transition from one stationary motion to another), to distinguish which two signs were necessary, the former does not here come into consideration. Dividing, moreover, the equation by T, it reads:—

$$\frac{dQ}{T} = dS.$$

Supposing this equation integrated for a circular process, and considering at the same time that S has the same value at the end of the circular process as at the beginning, we obtain

$$\int \frac{dQ}{T} = 0. \quad . \quad . \quad . \quad . \quad . \quad (38)$$

This is the equation which I first published[†], in 1854, as an expression of the second axiom of the mechanical theory of heat

* Pogg. *Ann.* vol. cxxv. p. 353; *Abhandlungen über die mechanische Wärmetheorie*, vol. ii. p. 1.

† Pogg. *Ann.* vol. xciii. p. 353; *Abhandl. über die mech. Wärmetheorie*, vol. i. p. 127.

for convertible circular processes.   At that time I derived it from the maxim *that heat cannot of itself pass from a colder to a hotter body.*   I afterwards* derived the same equation in a very differ- ent way, namely from the law cited above, that *the work which can be done by heat in an alteration of the arrangement of a body is proportional to the absolute temperature,* in conjunction with the assumption *that the heat actually present in a body is dependent on its temperature only, and not on the arrangement of its consti- tuents.*   Therewith I considered the circumstance that in this way we could arrive at the already otherwise proved equation a main support of that law.   Now the preceding analysis shows how that law, and with it the second axiom of the mechanical theory of heat, can be reduced to general mechanical principles.

XXI. *Description of a Model of a Conoidal Cubic Surface called the "Cylindroid," which is presented in the Theory of the Geome- trical freedom of a Rigid Body.   By* Robert Stawell Ball, *A.M., Professor of Applied Mathematics and Mechanism, Royal College of Science for Ireland†.*

WE become acquainted with the geometrical freedom which a rigid body enjoys by ascertaining the character of all the displacements which the nature of the restraints will permit the body to accept.   If a displacement be infinitely small, it is produced by screwing the body along a certain screw.   If a dis- placement have finite magnitude, it is produced by an infinite series of infinitely small screw displacements.   For the analysis of geometrical freedom we shall only consider infinitely small screw displacements.   This includes the initial stages of all dis- placements.

To analyze the geometrical restraints of a rigid body we pro- ceed as follows.   Take any line in space.   Conceive this line to be the axis about which screws are successively formed of every pitch from $-\infty$ to $+\infty$.   (The pitch of a screw is the distance its nut advances when turned through the angular unit.)   We endeavour successively to displace the body about each of these screws, and record the particular screw or screws, if any, about which the restraints have permitted the body to receive a dis- placement.   The same process is to be repeated for every other line in space.   If it be found that the restraints have not per- mitted the body to receive any one of these displacements, then the body is rigidly fixed in space.

If, after all the screws have been tried, the body be found capable of displacement about one screw only, the body possesses the lowest degree of freedom. If one screw (A) be discovered, and, the trials being continued, a second screw (B) be found, the remaining trials may be abridged by considering the information which the discovery of two screws affords.

The body may receive any displacement about one or both of the two screws A and B. The composition of these displacements gives a resultant which could have been produced by displacement about a single screw. The locus of this single screw is the conoidal cubic surface which has been called the "cylindroid" (at the suggestion of Professor Cayley). The equation of the surface is

$$z(x^2 + y^2) - 2axy = 0.$$

Any line (s) upon the surface is considered to be a screw, of which the pitch is

$$c + a \cos 2\theta,$$

where c is any constant, and θ is the angle between s and the line

$$y = 0,$$
$$z = 0.$$

The fundamental property of the cylindroid is thus stated. If any three screws of the surface be taken, and if a body be displaced by being screwed along each of these screws through a small angle proportional to the sine of the angle between the remaining screws, the body after the last displacement will occupy the same position that it did before the first.

The equation of the cylindroid is thus deduced. Take as axes of x and y two screws intersecting at right angles whose pitches are $c + a$ and $c - a$; a body is rotated about each of these screws through angles $\omega \cos \theta$, $\omega \sin \theta$ respectively. The corresponding translations are $(c + a) \omega \cos \theta$, and $(c - a) \omega \sin \theta$. The resultant of the translations may be resolved into two components, of which $(c + a \cos 2\theta) \omega$ is parallel to the resultant of the rotations, and $a\omega \sin 2\theta$ is perpendicular to the same line. The latter component has merely the effect of transferring in a normal plane the resultant of the rotations to a distance $a \sin 2\theta$, the resultant moving parallel to itself. The two original screw-movements are therefore compounded into a single screw whose pitch is $c + a \cos 2\theta$. The position of the screw is defined by the equations

$$\begin{cases} y = x \tan \theta, \\ z = a \sin 2\theta. \end{cases}$$

Eliminating θ, we have the equation of the surface.

The property of the surface is thus proved. Let θ_1, θ_2, θ_3 be the angles of three screws upon the surface, and ω_1, ω_2, ω_3 be the displacements about them. Each of these displacements may be resolved into screw-displacements about the screws of x and y. The conditions necessary and sufficient for the displacements to neutralize each other are

$$\omega_1 \sin \theta_1 + \omega_2 \sin \theta_2 + \omega_3 \sin \theta_3 = 0,$$
$$\omega_1 \cos \theta_1 + \omega_2 \cos \theta_2 + \omega_3 \cos \theta_3 = 0.$$

Thus each rotation is proportional to the sine of the angle between the other two screws.

For the complete determination of the cylindroid and the pitch of all its screws, we must have the quantities a and c. These quantities, as well as the position of the cylindroid in space, are completely determined when two screws of the system are known.

In the model of the cylindroid which is exhibited, the parameter a is 2·6 inches. The wires which correspond in the model with the generating lines of the surface represent the axes of the screws. The distribution of pitch upon the generating lines is shown by colouring a length of $2 \cdot 6 \times \cos 2\theta$ inches upon each wire. The distinction between positive and negative pitches is indicated by colouring the former red and the latter black. This model is in the possession of the London Mathematical Society, 22 Albemarle Street.

It is remarkable that the addition of any constant (c) to all the pitches attributed in the model to the screws does not affect the fundamental property of the cylindroid.

When a rigid body is found capable of being displaced about a pair of screws, it is necessarily capable of being displaced about every screw on the cylindroid determined by that pair.

The theorem of the cylindroid includes as particular cases the well-known rules for the composition of two displacements parallel to given lines, or of two small rotations about intersecting axes. If the parameter a be zero, the cylindroid reduces to a plane, and the pitches of all the screws become equal. If the arbitrary constant (c) which expresses the pitch be infinite, we have the theorem for displacements; and if the pitch be zero we have the theorem for rotations. As far as the composition of two displacements is concerned, the plane can only be regarded as a degraded form of the cylindroid, from which the most essential feature has disappeared.

Royal College of Science,
. July 1871.

XXII. *On the steady Flow of a Liquid.* *By* Henry Moseley, *M.A., D.C.L., Canon of Bristol, F.R.S., Corresponding Member of the Institute of France, &c.**

THE hydraulic experiments of M. Darcy, continued after his death by M. Bazin, were made with remarkable industry and scientific skill, and on a large scale. In the first series † circular pipes were used, and they were placed horizontally. In the second ‡ the channels were rectangular, open and closed, and they were sloped. The experiments of the first series, which are those referred to in the following paper, were made with pipes of different materials, in different states of roughness or smoothness of internal surface, and of different diameters, from $\frac{1}{2}$ inch to 20 inches. Their lengths were generally 120 yards, but some of them 60 yards; and the water was made to traverse them with velocities varying from 1 inch per second to 20 feet.

All the necessary precautions were taken to determine the mean diameters of these pipes, and to measure the water discharged from them. To feed them, it was received from the reservoirs at Chaillot into a cylindrical vessel 28 metres high, in which it could be made to stand at any required height by opening more or less a cock in the supply-pipe. It passed from the bottom of this reservoir by means of a horizontal pipe 300 metres long, into a great horizontal cylinder, 1 metre in diameter and $3\frac{1}{2}$ metres long, to one end of which horizontal cylinder were adjusted the pipes to be experimented upon. This cylinder was crossed internally by an iron diaphragm pierced with small holes, through which the water was made to pass that its *vis viva* might (as far as possible) be destroyed before it entered the pipes experimented upon. Pressure-gauges were fixed at four different points of each pipe—the first being placed near the end by which the water escaped from the pipe into the reservoir of efflux, the second at 50 metres from it, the third at 100 metres from the first, the fourth near the point of entrance of the water from the horizontal cylinder into the pipe at 4·7 metres from the third, and the fifth in the horizontal pipe. They were water-gauges.

The first and third gauges being 100 metres apart, the difference of the heights of the water in these gauges showed, when the flow of the water had become steady, the head of water necessary to overcome the resistances opposed to the flow of that portion of the water in the pipe which intervened between these

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* Communicated by the Author.

† *Recherches Expérimentales relatives au mouvement de l'eau dans les Tuyaux.* Paris: Bachelier, 1857.

‡ *Recherches Hydrauliques,* par M. Darcy. Continuées par M. Bazin. Paris: Dunot, 1865.

two gauges. This head of water is that designated in the following paper by the symbol h.

Besides determining the efflux under different conditions, M. Darcy determined also the velocities of the water at different distances from the axis of certain of the pipes on which he experimented; and with reference to the theory of the flow of liquids, this was the most interesting feature of his experiments. He effected it by means of the instrument well known as Pitot's tube, into the construction and use of which he introduced some admirable improvements, for the particulars of which the reader is referred to his work. The results he arrived at are stated at length in Table I. of the following paper.

A film in a liquid flowing through a pipe, in the sense in which the word is used in the following paper, is a continuous portion of the liquid, every molecule in which flows with the same velocity. A filament is an exceedingly narrow film. To the surface of the pipe a film of the liquid is supposed to adhere and to remain at rest. The film adjacent to it moves over this fixed film, the third over the second, the fourth over the third, &c. with continually increasing velocities; the film nearer to the surface moving always slower than that more remote. This is proved by the experiments of MM. Darcy and Bazin.

The resistance opposed by the surface of the pipe to the flow of the liquid immediately in contact with it is represented by the formula

$$P = \mu_1 + \lambda_1 V^2, \quad \cdots \quad \cdots \quad (1)$$

where P is the resistance per unit of surface, V the velocity of the flow, λ_1 a constant, and μ_1 a term as to which there is a difference of opinion whether it is constant or increases with the velocity.

This formula is founded on experiment[*]. The first term in it is considered to represent that part of the resistance which is due to the adherence of the liquid to the surface of the pipe, and which is of the nature of that which in solid bodies is opposed to shearing.

The second term is understood to represent the resistance caused by the impacts of the molecules of the flowing liquid on those of the film of liquid fixed to the surface of the pipe, and on the eminences of the solid surface of the pipe which project through that film.

Let the steady flow of a liquid in a horizontal circular pipe of uniform dimensions and roughness of surface be supposed to be maintained by the pressure of the liquid in a reservoir whose surface is always on the same level; let

[*] See Poncelet, *Introduction à la Mécanique Industrielle*, art. 387, 388.

U = work done per unit of time on the liquid which enters the pipe by the pressure of that in the reservoir,

U_1 = work carried away per unit of time by the liquid which flows from the extremity of the pipe,

U_2 = work expended on the various resistances which are opposed to the descent of the liquid in the reservoir and to its passage from the reservoir through its aperture into the pipe,

U_3 = work expended on the resistance of the internal surface of the pipe to the flow of the liquid along it,

U_4 = internal work of the resistance of the films to the flowing of each film over the surface of the next in succession;

then, by the principle of virtual velocities,

$$U = U_1 + U_2 + U_3 + U_4. \quad . \quad . \quad . \quad . \quad (2)$$

Let v = velocity of any film,

v_0 = velocity of the filament which coincides with the axis of the pipe,

V = velocity of the film which is in contact with the surface of the pipe,

R = internal radius of pipe,

r = radius of the film whose velocity is v,

ρ = resistance per unit of surface to the sliding of the film whose radius is r over that whose radius is $r + dr$,

h = height of the liquid in the reservoir above the centre of the aperture,

l = length of the pipe,

w = weight of cubic unit of liquid.

Let the unit of length in all the above measurements be the French metre, and the unit of weight the French kilogramme.

The weight of the liquid which flows out of the pipe per unit of time is represented by

$$\int_0^R wv(2\pi r dr).$$

This weight of liquid is therefore that which descends through the height h in the reservoir per unit of time;

$$\therefore \ U = h \int_0^r wv(2\pi r dr) = 2\pi wh \int_0^R vr dr. \quad . \quad . \quad (3)$$

Also the work U_1 which the liquid flowing out of the pipe carries away with it per unit of time, is represented by half its *vis viva*.

But the weight of liquid which flows per unit of time between two films whose radii are r and $r + dr$ is $w(2\pi r dr)v$. Half the

vis viva of this liquid is therefore $\dfrac{w(2\pi r dr)v}{2g}v^2$;

$$\therefore \ U_1 = \int_0^R \frac{w(2\pi r dr)v}{2g}v^2 = \frac{\pi w}{g}\int_0^R v^3 r\,dr. \quad . \quad . \quad (4)$$

The work U_3 expended per unit of time on the resistance of the internal surface of the pipe to the flow of the liquid is equal to the entire resistance of the surface multiplied by the velocity V of the liquid in contact with it, since V is the distance through which that resistance is overcome per unit of time. But the resistance of the pipe per unit of surface is

$$\mu_1 + \lambda_1 V^2 \text{ (see equation 1)},$$

and the surface is $2\pi R l$;

$$\therefore \ U_3 = 2\pi R l(\mu_1 + \lambda_1 V^2)\,V. \quad . \quad . \quad . \quad (5)$$

To determine U_4, which represents the aggregate internal work of the mutual resistances of the successive films of liquid, let it be observed that, as v represents the velocity of the film whose radius is r, $v - \left(\dfrac{dv}{dr}\right)dr$ represents that of the film whose radius is $r + dr$, the negative sign being taken because as r increases v diminishes. The distance by which one film slips over the next in the unit of time is therefore represented by $-\left(\dfrac{dv}{dr}\right)dr$. But the resistance opposed to this slipping is $2\pi r l \rho$,

$$\therefore \ U_4 = -\int_0^R 2\pi r l \rho \left(\frac{dv}{dr}\right)dr = -2\pi l \int_0^R \rho r\left(\frac{dv}{dr}\right)dr.$$

To determine the unit of resistance ρ which is opposed by the film whose radius is $r + dr$ to the motion over it of that whose radius is r, let the velocity $v - \left(\dfrac{dv}{dr}\right)dr$ of the former film be supposed to be communicated in an opposite direction to both. The resistance of the one film opposed to the motion over it of the other will not thus be changed, but the former will be brought to rest, and the other will move over it with the velocity $-\dfrac{dv}{dr}dr$. The case will thus become the same with that of the film which moves in contact with that fixed to the internal surface of the pipe, except that the constants μ_1 and λ_1 will have different values. Let these values be μ and λ, then

$$\rho = \mu + \lambda\left[\left(\frac{dv}{dr}\right)dr\right]^2;$$

$$\therefore \ U_4 = -2\pi l \int_0^R \left\{\mu + \lambda\left[\left(\frac{dv}{dr}\right)dr\right]^2\right\} r\left(\frac{dv}{dr}\right)dr,$$

in which the second term may be neglected as of infinitely small

dimensions compared with the first,

$$\therefore U_4 = -2\pi l \int_0^R \mu r \left(\frac{dv}{dr}\right) dr; \quad . \quad . \quad . \quad . \quad (6)$$

$\therefore$ by equation (2),

$$2\pi wh \int_0^R vr\,dr$$
$$= \frac{\pi w}{g} \int_0^R v^3 r\,dr + U_2 + 2\pi l R (\mu_1 + \lambda_1 V^2) V - 2\pi l \int_0^R \mu r \left(\frac{dv}{dr}\right) dr;$$

or considering separately the case of a portion of the liquid bounded by a film whose radius is r,

$$2\pi wh \int_0^r vr\,dr = \frac{\pi w}{g} \int_0^r v^3 r\,dr + U_2 - 2\pi l \int_0^r \mu r \left(\frac{dv}{dr}\right) dr,$$

in which equation the work of the bounding film of the portion of the liquid which is considered separately from the rest, is included in the last integral. Differentiating the above equation, considering U_2 constant, and reducing,

$$2ghv = v^3 - \frac{2gl\mu}{w} \left(\frac{dv}{dr}\right). \quad . \quad . \quad . \quad . \quad (7)$$

If there were no resistance to the flow of the films over one another, or to the flow of the liquid over the internal surface of the pipe, the whole of the work done by the weight of the liquid in the reservoir on that in the pipe per unit of time would be accumulated in the liquid discharged per unit of time. Let v be the velocity the liquid would under these circumstances acquire. Then $\pi R^2 v$ represents the discharge per unit of time, and $\dfrac{w\pi R^2 v}{2g} v^2$ represents the work accumulated in it. Also $hw\pi R^2 v$ represents the work done upon it per unit of time by the pressure of the liquid in the reservoir,

$$\therefore \frac{w\pi R^2 v}{2g} v^2 = hw\pi R^2 v;$$

$$\therefore v^2 = 2gh. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The same result is arrived at, as it ought to be, by making $\mu = 0$ in equation (7), and substituting v for v. By equation (7),

$$v^2 v - v^3 = -- \frac{2gl\mu}{w} \left(\frac{dv}{dr}\right),$$

$$\therefore \left(\frac{dv}{dr}\right) \frac{1}{v(v^2 - v^2)} = -\frac{w}{2gl\mu}$$

But

$$\frac{1}{v(v^2 - v^2)} = \frac{1}{v^2} \left[\frac{1}{v} + \frac{1}{2}\left(\frac{1}{v-v} - \frac{1}{v+v}\right)\right];$$

$$\therefore \left(\frac{dv}{dr}\right)\left[\frac{1}{v} + \frac{1}{2}\left(\frac{1}{v-v} - \frac{1}{v+v}\right)\right] = -\frac{wv^2}{2gl\mu} = -\frac{wh}{\mu l}.$$

Let $\frac{h}{l} = i$. Then integrating between the limits 0 and r, and assuming μ *not* to be a function of r,

$$\log_\epsilon\left(\frac{v}{v_0}\right) - \frac{1}{2}\left(\log_\epsilon\frac{v+v}{v-v_0} + \log_\epsilon\frac{v+v}{v+v_0}\right) = -\frac{wi}{\mu}\,r\,;$$

$$\therefore \log_\epsilon\left(\frac{v}{v_0}\right) - \log_\epsilon\left(\frac{v^2-v^2}{v^2-v_0^2}\right)^{\frac{1}{2}} = -\frac{wir}{\mu}\,;$$

$$\therefore \log_\epsilon\left[\left(\frac{v}{v_0}\right)\left(\frac{v^2-v_0^2}{v^2-v^2}\right)^{\frac{1}{2}}\right] = -\frac{wir}{\mu}\,;$$

$$\therefore \left(\frac{v}{v_0}\right)\left(\frac{v^2-v_0^2}{v^2-v^2}\right)^{\frac{1}{2}} = \epsilon^{-\frac{wir}{\mu}}\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$\therefore \frac{\left(\frac{v}{v_0}\right)^2-1}{\left(\frac{v}{v}\right)^2-1} = \epsilon^{-\frac{2wir}{\mu}}\,,$$

$$\left(\frac{v}{v}\right)^2-1 = \left[\left(\frac{v}{v_0}\right)^2-1\right]\epsilon^{\frac{2wir}{\mu}}\,,$$

$$v^2 = \frac{v^2}{1+\left[\left(\frac{v}{v_0}\right)^2-1\right]\epsilon^{\frac{2wir}{\mu}}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

In certain of the experiments of M. Darcy, the velocities of films of water were determined at three different distances (r) from the axes of pipes of diameters varying from 0·188 to 0·500 metre, under heads of water (h) varying from 0·202 to 13·427 metres, the values of v corresponding to which varied from 1·18 to 16·56 metres. Throughout this range of experiments the values of $\left(\dfrac{v}{v_0}\right)$ were for the same values of r approximately the same.

This will be seen by inspecting the following Table I., in the two central columns of which are given the values of the ratio $\left(\dfrac{v}{v_0}\right)$ in respect of the first, second, and fourth films, as determined by the experiments of M. Darcy in pipes of different diameters, and of different degrees of roughness internally, and under different heads of water. These values of $\left(\dfrac{v}{v_0}\right)$ are calculated in respect of all the experiments of M. Darcy in which he has determined the values of v and v_0; and in all, the value of that fraction is approximately the same for the same value of r.

Table I.

New cast-iron pipe. Diameter 0·188 metre. Surface cleaned (Darcy, p. 145).

Index number.	h.	v.	By experiment. r. 0·0643 (v_2)	0·0637 (v_2)	0·0325 (v_1)	0. (v_0)	$\left(\frac{v_1}{v_0}\right)$	$\left(\frac{v_3}{v_0}\right)$	By theory, $v = v_0 - 2 \cdot 25\, r.$ r. 0·0643 (v_0)	0·0637 (v_2)	0·0325 (v_1)	0. (v_0)	By experiment. Discharge per 1″ in litres.	Mean velocity, in metres.
159.	0·368	2·69	0·758	0·710	0·845	0·885	·9548	·8564	0·766	0·768	0·825	0·885	1263·3	0·758
160.	0·865	3·975	1·128	1·131	1·244	1·303	·9547	·8657	1·126	1·129	1·2234	1·303	1878·2	1·128
161.	1·34	5·13	1·488	1·463	1·673	1·747	·9576	·8517	1·569	1·513	1·6403	1·747	2479·2	1·488
162.	2·225	6·61	1·933	1·957	2·174	2·280	·9535	·8478	1·9706	1·982	2·14106	2·280	3219·4	1·933
163.	3·810	8·14	2·506	2·515	2·798	2·922	·9575	·8576	2·522	2·531	2·7435	2·922	4173·6	2·506
164.	10·980	14·67	4·323	4·310	4·876	5·082	·9594	·8506	4·3863	4·104	4·7715	5·082	7191·7	4·323

Cast-iron pipe, incrusted with deposit. Diameter 0·2432 metre.

Index number.	h.	v.	By experiment. r. 0·088 (v_3)	0·0838 (v_2)	0·044 (v_1)	0. (v_0)	$\left(\frac{v_1}{v_0}\right)$	$\left(\frac{v_3}{v_0}\right)$	By theory. r. 0·088 (v_3)	0·0838 (v_2)	0·044 (v_1)	0. (v_0)	By experiment. Discharge per 1″ in litres.	Mean velocity, in metres.
167.	0·202	1·99	0·456	0·452	0·538	0·576	·9356	·7930	0·4717	0·4760	0·5218	0·5550	1259·3	0·452
168.	0·473	3·05	0·694	0·707	0·811	0·876	·9258	·7922	0·7187	0·7252	0·7949	0·876	1972·4	0·707
170.	2·290	6·71	1·489	1·547	1·753	1·883	·9309	·7907	1·545	1·558	1·705	1·883	4309·4	1·547
171.	3·200	7·93	1·771	1·833	2·118	2·273	·9318	·7790	1·865	1·881	2·058	2·273	5111·5	1·833
173.	13·981	16·56	3·689	3·833	4·275	4·593	·9346	·8034	3·768	3·802	4·160	4·593	10685·0	3·833

Cast iron, cleaned.   Diameter 0·2447 metre.

Mean velocity, in metres.	Discharge per 1'', in litres.	0. (v_0)	0·044. (v_1)	0·0843. (v_2)	0·088. (v_3)	$\left(\frac{v_3}{v_0}\right)$	$\left(\frac{v_1}{v_0}\right)$	0. (v_0)	0·044. (v_1)	0·0843. (v_2)	0·088. (v_3)	v.	h.	Index number.
0·537	1513·7	0·630	0·571	·521	0·517	·8444	·9643	0·630	0·601	0·537	0·531	1·96	0·195	175.
		1·103	0·998	0·9137	0·9063	·8313	·9474	1·103	1·045	0·949	0·917	3·41	0·503	176.
1·904	10736·5	2·230	2·020	1·844	1·829	·8412	·9439	2·230	2·105	1·904	1·877	6·88	2·41	178.
4·497	12682·0	5·212	4·721	4·312	4·276	·8478	·9468	5·212	4·935	4·497	4·419	16·237	13·427	181.

Diameter 0·297 metre.

0. (v_0)	0·052. (v_1)	0·102. (v_2)	0·1023. (v_3)	$\left(\frac{v_3}{v_0}\right)$	$\left(\frac{v_1}{v_0}\right)$	0. (v_0)	0·052. (v_1)	0·102. (v_2)	0·1023. (v_3)	v.	h.	Index number.
0·435	0·357	0·346	0·345	·8160	·9425	0·435	0·410	0·355	0·355	1·180	0·070	
1·418	1·262	1·161	1·126	·8716	·9562	1·418	1·356	1·230	1·236	3·480	0·617	
1·931	1·708	1·535	1·534	·8622	·9522	1·931	1·839	1·666	1·665	4·700	1·125	
2·708	2·617	2·152	2·151	·8733	·9564	2·708	2·590	2·355	2·363	6·650	2·251	

Cast iron, new.   Diameter 0·5 metre.

Mean velocity, in metres.	Discharge per 1'', in litres.	0. (v_0)	0·090. (v_1)	0·170. (v_2)	0·1723. (v_3)	$\left(\frac{v_3}{v_0}\right)$	$\left(\frac{v_1}{v_0}\right)$	0. (v_0)	·090. (v_1)	0·170. (v_2)	0·1723. (v_3)	v.	h.	Index number.
0·4752	5598·6	0·571	0·466	0·389	0·387	·8319	·9369	0·571	0·535	0·477	0·475	1·22	0·075	192.
0·7951	9366·8	0·919	0·751	0·627	0·624	·8650	·9455	0·919	0·869	0·796	0·795	1·88	0·180	194.
1·1197	13191·8	1·319	1·077	0·901	0·895	·8189	·9469	1·319	1·249	1·133	1·197	1·37	0·096	197.

From this it follows that, however different the actual velocities of the films may be by reason of differences in the heads of water and in the diameters and internal roughnesses of the pipes, their relative velocities at equal distances from the axis are (in the same liquid) approximately the same. Since moreover $\left(\dfrac{v}{v_0}\right)$ is approximately independent of these differences in the heads of water and the diameters and roughnesses of the pipes, it follows from equation (9) that $\left(\dfrac{v^2-v_0^2}{v^2-v^2}\right)^{\frac{1}{2}}$ and $\dfrac{wi}{\mu}$ are also independent of them, and that they must have approximately the same values for the same values of r in respect of all the heads of water, and all the kinds and diameters of pipes included in the range of M. Darcy's experiments.

This will be found to be the case. Table II. contains the values of $\left(\dfrac{v^2-v_0^2}{v^2-v^2}\right)^{\frac{1}{2}}$ calculated with respect to the characteristic experiments of each group; and its value will be seen to be in every case, approximately, *unity*; and, that being admitted, Table I. shows that the values of v calculated from equation (9) agree with experiment when $\dfrac{wi}{\mu}$ is taken equal to the constant $2\cdot25$; whilst Table III. shows the *efflux* also to correspond with experiment when calculated on that supposition.

Table II.

Index number	Diameter of pipe.	v.	v_0.	v_1.	v_2.	v_3.	Values of $\left(\dfrac{v^2-v_0^2}{v^2-v^2}\right)^{\frac{1}{2}}$, corresponding to		
							v_1.	v_2.	v_3.
159.	0·188	2·69	0·885	0·845	0·760	0·758	·99513	·98375	·9835
171.	0·2432	8·14	2·273	2·118	1·833	1·771	·9944	·9855	·9835
181.	0·2447	14·91	5·212	4·935	4·497	4·419	·99288	·98268	·98212
	0·297	1·18	0·435	0·410	0·355	0·355	·99131	·97173	·97173
187.	0·297	6·65	2·708	2·590	2·355	2·365	·99161	·97662	·97662
192.	0·50	1·09	0·571	0·535	0·477	0·475	·97767	·94734	·92847
197.	0·50	2·26	1·319	1·249	1·133	1·197	·97433	·93848	·93482

Since in M. Darcy's experiments it thus appears that, approximately,

$$\left(\frac{v^2-v_0^2}{v^2-v^2}\right)^{\frac{1}{3}}=1, \quad \ldots \ldots \quad (11)$$

it follows from equation (9) that, approximately,

$$v=v_0\epsilon^{-\frac{wir}{\mu}}. \quad \ldots \ldots \quad (12)$$

This formula may be proved independently as follows :—$2\pi rl$ being the surface of a film whose radius is r, and μ the resistance per unit of surface to its being made to move over the film whose radius is $r + dr$, $2\pi rl\mu$ represents the whole resistance to its being so moved. And since the distance it is moved over it in the unit of time is represented by $-\left(\dfrac{dv}{dr}\right)dr$, the whole work, per unit of time, of that resistance is represented by

$$-2\pi l\mu r\left(\frac{dv}{dr}\right)dr.$$

Also, since h is the head of water and the vertical section of the film which receives the constant pressure of this head of water is represented by $(2\pi r dr)$, the pressure constantly acting to give motion to the film is $(2\pi r dr)wh$. But in the unit of time this pressure acts through the distance v, because the film moves through that distance. Therefore the work per unit of time of the pressure which gives motion to the film is represented by

$$2\pi whvr dr.$$

Therefore, since the motion is uniform, by the principle of virtual velocities,

$$-2\pi l\mu r\left(\frac{dv}{dr}\right)dr = 2\pi whvr dr\ ;$$

$$\therefore\ \frac{\left(\dfrac{dv}{dr}\right)}{v} = -\frac{wh}{\mu l} = -\frac{wi}{\mu}\ ;$$

$$\therefore\ \log_{\epsilon}\left(\frac{v}{v_0}\right) = -\frac{wir}{\mu},$$

$$v = v_0\epsilon^{-\frac{wi}{\mu}}.$$

In this investigation the accumulation of work in the liquid which escapes per unit of time is neglected. It is so far generally incorrect; and therein lies the difference between the values of v as determined by equations (10) and (12). It would be absolutely true if, by the pressure of an additional head of water, that in the horizontal pipe had been made to enter it with the velocity with which it eventually leaves it, the pressure of the head h only acting to overcome the resistances to its motion through the horizontal pipe, and so to preserve in it the work with which it entered it and with which it leaves it. Now this was nearly the case in the experiments of M. Darcy, so that equation (12), not absolutely true in the general case, is approximately true in the special case of his experiments. By

the expedients he adopted, the water was made to arrive at the third pressure-gauge with its velocity already acquired, which velocity was maintained in it against the resistance of the horizontal pipe up to the first gauge by a pressure which is represented by the difference h of the heights of the water in the third and first gauges. The additional head of water which communicated to it this velocity before it reached the third gauge was represented by the difference between the heights of water in that gauge and in the fifth gauge.

Since $\left(\dfrac{v}{v_0}\right)$ was approximately independent of the kinds and dimensions of the pipes used in M. Darcy's experiments and of the heads of water, and dependent only on the value of r, it follows that in those experiments $\dfrac{wi}{\mu}$ was constant. If this be the case, then determining its value as given by any experiment and representing it by $\gamma*$, the relation of the velocity v of any film whose radius is r to that r_0 of the central filament will be represented in *all* the experiments approximately by the equation

$$v = v_0 \epsilon^{-\gamma r}. \quad . \quad . \quad . \quad . \quad (13)$$

This supposes the values of v and v_0 to have been determined in that experiment with perfect accuracy. That cannot, however, have been the case in any experiment. Admirable as are the experiments of M. Darcy, and far surpassing any others in accuracy that have ever been attempted, the means used (the only ones perhaps possible) are in their nature defective. He has applied corrections to his results; but the authority of these is perhaps sometimes doubtful, and it is rather the general laws observable in the experiments than the precise results on which reliance is to be placed. On this point I will quote his own words :—" Les quelques différences qui existent ne peuvent être attribuées qu'à la difficulté d'obtenir plus de précision dans les expériences. La moindre erreur, en effet, dans l'indication des instruments qui donnent les vitesses entières doit influer d'une manière très-sensible sur les différences des vitesses si l'erreur ne porte pas à la fois sur les deux vitesses." (P. 152.)

In the experiments of M. Darcy the lengths of the pipes between the first and third gauges were 100 metres. It is only to this portion of each pipe that the velocities recorded in Table I. refer.

* $\gamma = \dfrac{wi}{\mu} = \dfrac{wh}{\mu l} = \dfrac{wv^2}{2g\mu l}$ (equation 8),

$\therefore \mu = \dfrac{wi}{\gamma} = \dfrac{wh}{\gamma l} = \dfrac{wv^2}{2g\gamma l}.$

To determine approximately the value of γ, I have taken the group of experiments 160. I have no reason to believe that these were made with greater accuracy than any others. The value of γ which I have obtained from them is indeed certainly too great. I have obtained it as follows:—

By those experiments

$$r_3 = \cdot0643, \quad v_0 = 1\cdot303, \quad v_3 = 1\cdot128;$$

$\therefore$ by equation (13)

$$1\cdot128 = 1\cdot303\epsilon^{-\cdot0643\gamma};$$

$$\therefore \epsilon^{\cdot0643\gamma} = \frac{1303}{1128}, \text{ and } \gamma = \frac{\log_\epsilon\left(\dfrac{1343}{1128}\right)}{\cdot0643}.$$

But

$$\log_\epsilon\left(\frac{1343}{1128}\right) = \cdot14422315;$$

$$\therefore \gamma = \frac{\cdot14422315}{\cdot0643} = 2\cdot2435 = 2\cdot25 \text{ nearly};$$

$$\therefore v = \epsilon^{-2\cdot25r}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

The values of v_1, v_2, v_3, as calculated by this formula for eighty-eight experiments, are given in Table I., and also the corresponding values as given by experiment. The accordance is approximative only, but it is undeniable. It is least apparent in the last group of experiments, where the diameter of the pipe was the largest ($\frac{1}{2}$ a metre), and where the head of water was small and the velocity low. In those experiments the errors of observation were likely to be the greatest. In all, the difference between theory and experiment is greater at distances near the axis of the pipe than further off. I do not know whether the chance of errors of experiment is greater there than elsewhere. Experiments on the efflux from a pipe are in their nature more reliable than any on the velocities of the liquid at different distances from its axis can be.

To determine approximately the efflux in terms of the velocity v_0 of the central filament, let Q_R represent the volume of liquid which flows per unit of time from a pipe whose radius is R and length l under the head of water h,

$$Q_R = \int_0^R (2\pi r\,dr)v = 2\pi \int_0^R vr\,dr;$$

or, by equation (12),

$$Q_R = 2\pi v_0 \int_0^R \epsilon^{-\frac{vir}{\mu}} r\,dr. \quad . \quad . \quad . \quad . \quad (15)$$

O 2

But

$$\int \epsilon^{-\frac{wir}{\mu}} r\,dr = -\left(\frac{\mu}{wi}\right)\epsilon^{-\frac{wir}{\mu}} r + \left(\frac{\mu}{wi}\right)\int \epsilon^{-\frac{wir}{\mu}} dr,$$

$$\int \epsilon^{-\frac{wir}{\mu}} r\,dr = -\left(\frac{\mu}{wi}\right)\epsilon^{-\frac{wir}{\mu}} r - \left(\frac{\mu}{wi}\right)^2 \epsilon^{-\frac{uir}{\mu}} + C \, ;$$

$$\therefore \int_0^R \epsilon^{-\frac{wir}{\mu}} = -\left(\frac{\mu}{wi}\right)\epsilon^{-\frac{wiR}{\mu}} R - \left(\frac{\mu}{wi}\right)^2\left(\epsilon^{-\frac{wiR}{\mu}} - 1\right)$$

$$=\left(\frac{\mu}{wi}\right)^2\left[1 - \left(1 + \frac{wiR}{\mu}\right)\epsilon^{-\frac{wiR}{\mu}}\right];$$

$$\therefore \ Q_R = 2\pi\left(\frac{\mu}{wi}\right)^2\left[1 - \left(1 + \frac{wiR}{\mu}\right)\epsilon^{-\frac{wiR}{\mu}}\right]v_0. \quad . \quad (16)$$

If we assume approximately as before,

$$\frac{wi}{\mu} = 2{\cdot}25,$$

$$Q_R = \frac{2\pi}{(2{\cdot}25)^2}\left[1 - (1 + 2{\cdot}25\,R)\epsilon^{-2{\cdot}25R}\right]v_0. \quad . \quad (17)$$

In the following Table III. various experiments of M. Darcy on the volumes of water which flow in given times through pipes of different materials and states of their internal surfaces and different diameters, and under different heads of water, are compared with the above formula.

TABLE III.

New cast-iron pipe, interior clean.   Diameter 0·188 metre.

Index number.	Velocity of the water at the axis of the pipe, v_0.	Discharge per second in cubic metres by experiment (Darcy, p. 59).	Discharge per second in cubic metres by theory, $Q = \dfrac{2\pi v_0}{(2{\cdot}25)^2}\left[1-(1+2{\cdot}25\,R)\epsilon^{-2{\cdot}25R}\right]$.
159.	0·885	0·021664	0·021332
160.	1·303	0·031303	0·031391
161.	1·747	0·04132	0·042088
162.	2·280	0·05635	0·054928
163.	2·922	0·06956	0·070395
164.	5·082	0·11953	0·012243

Old cast-iron pipe, interior covered with deposit.   Diameter 0·2432 metre.

167.	0·575	0·02099	0·022377
168.	0·876	0·03287	0·034029
170.	1·883	0·07182	0·073147
171.	2·273	0·08519	0·088297
173.	4·593	0·17808	0·17842

The velocity v of any film as represented by equation (12), and the efflux Q_R as represented by equation (17), are given in terms of the velocity r_0 of the central filament, and can only be determined by these formulæ when that velocity is known. The experiments of M. Darcy having made it known in certain cases, they serve to verify the theory as it regards the relation of the efflux per unit of time to the velocity of the central filament. If that velocity could be represented in terms of the head of water and the diameter and length and degree of roughness of the pipe, then the formula by which this was represented being substituted for v_0 in equation (12) would complete the investigation. I propose to make it the subject of a future communication.

XXIII. *On a supposed new Integration of Differential Equations of the Second Order.* By Professor CAYLEY, F.R.S.[*]

I CANNOT assent to the views taken by Professor Challis in his paper in the July Number, " On the Application of a new Integration of Differential Equations of the Second Order to some unsolved Problems in the Calculus of Variations."

In any problem of the calculus of variations, where there are two variables x, y, the condition for a maximum or minimum is obtained in the form

$$\Lambda(\delta y - p\delta x) = 0 ;$$

and if the problem involves no relation between δx and δy, Professor Challis says that " we have with equal reason $\Lambda = 0$ and $\Lambda p = 0$;" and he goes on to argue that " it cannot be antecedently affirmed that these are identical equations ;" and further, that " it is necessary to take account of results deducible from them either separately or conjointly."

I object to this statement; it seems to me that in order that $\Lambda(\delta y - p\delta x)$ may vanish, the *only* condition is $\Lambda = 0$; we are not concerned with the equation $\Lambda p = 0$, as such, at all. But in certain cases it happens that p is a multiplier of the differential equation $\Lambda = 0$; viz. by writing this equation under the form $\Lambda p = 0$ we have an equation integrable *per se*, and which by its integration gives the integral of the differential equation $\Lambda = 0$.

Professor Challis, taking the view that the two equations are distinct from each other, considers (Problem II.) the following question :—" Required the minimum surface generated by the revolution of a line joining two given points in a plane passing

through the axis of revolution." Here

$$\Lambda = \frac{1}{\sqrt{1+p^2}} - \frac{yq}{(1+p^2)^{\frac{3}{2}}}.$$

The equation $\Lambda p = 0$ is integrable *per se*; and its integral is $\dfrac{y}{\sqrt{1+p^2}} = c$, or say $y = c\sqrt{1+p^2}$, being, as is known, the differential equation of a *catenary* having its directrix coincident with the axis of revolution; in fact the integral is

$$y = \frac{c}{2} \left(e^{\frac{x+c'}{c}} + e^{-\frac{x+c'}{c}} \right).$$

But there are difficulties as regards the application of this integral to the problem in hand; and Professor Challis is led to consider that the solution of the problem " can be effected only by taking into account an *independent* integration of $\Lambda = 0$;" and this he proceeds to obtain by a method which " consists essentially in first finding, when it is possible, the *evolute* of the curve or curves of which $\Lambda = 0$ is the differential equation, and then employing the involutes thence derivable, which may be regarded as the solution of the equation, to satisfy, either by computation or by graphical construction, the given conditions of the problem." This is perfectly allowable; but after the evolute is obtained, we must take not *any* involute, but the proper involute of such evolute; we thus have a solution of the differential equation $\Lambda = 0$, *the same* as is obtained by what Professor Challis considers to be the integration of the other equation $\Lambda p = 0$. This seems to me obvious *à priori*; but I will verify it in regard to the problem in hand. Taking, with Professor Challis, x', y' as the coordinates of that point of the evolute which is the centre of curvature at the point of the involute whose coordinates are x, y, and writing also $c = 2k$ (the equation in question is obtained in his paper), the several equations obtained by him are in effect as follows :—

$$y = \tfrac{1}{2} y',$$
$$x = x' - \frac{y'}{2p'},$$
$$x' + c' = -\frac{y'}{2} \sqrt{\frac{y'^2}{4c^2} - 1} - c \log\left(\frac{y'}{2c} + \sqrt{\frac{y'^2}{4c^2} - 1} \right),$$
$$\frac{1}{p'} = -\sqrt{\frac{y'^2}{4c^2} - 1}$$

(the third of these is his equation (α), taking therein the lower

signs and correcting an accidental or typographical error, viz. $\frac{y'}{2k}$ is printed instead of $\frac{y'}{2}$). But, from the foregoing equations, proceeding to eliminate x', y', p', we have

$$y' = 2y, \quad \frac{1}{p'} = -\sqrt{\frac{y^2}{c^2} - 1},$$

$$x' = \quad x - y\sqrt{\frac{y^2}{c^2} - 1},$$

$$x' = -c' - y\sqrt{\frac{y^2}{c^2} - 1} - c\log\left(\frac{y}{c} - \sqrt{\frac{y^2}{c^2} - 1}\right);$$

and thence

$$-\frac{x + c'}{c} = \log\left(\frac{y}{c} - \sqrt{\frac{y^2}{c^2} - 1}\right);$$

that is,

$$e^{-\frac{x+c'}{c}} = \frac{y}{c} - \sqrt{\frac{y^2}{c^2} - 1},$$

whence

$$e^{\frac{x+c'}{c}} = \frac{y}{c} + \sqrt{\frac{y^2}{c^2} - 1};$$

or, what is the same thing,

$$y = \frac{c}{2}\left(e^{\frac{x+c'}{c}} + e^{-\frac{x+c'}{c}}\right),$$

the before-mentioned integral of the differential equation

$$y = c\sqrt{1 + p^2},$$

which, according to Professor Challis, is the integral, not of the equation $A = 0$, but of the other equation $Ap = 0$.

Edinburgh, August 10, 1871.

XXIV. *On the General Circulation and Distribution of the Atmosphere.* By Professor J. D. EVERETT, *of Queen's College, Belfast**.

AT the Meeting of the British Association in Dublin in 1857 a paper was read by Professor James Thomson "On the Grand Currents of Atmospheric Circulation," in which certain new views were propounded, differing greatly from the popularly received theory of Lieutenant Maury†, and accounting for the

* Communicated by the Author, having been read to the British Association at Edinburgh (Section A), August 1871.

† The theory of atmospheric circulation which is adopted by Maury is

prevalent south-west winds in north temperate latitudes in a more satisfactory manner than by Maury's supposed crossing of the upper and lower currents near the Tropic of Cancer.

Three years later, Mr. W. Ferrel, A.M., Assistant upon the American Ephemeris and Nautical Almanack, published (in vols. i. and ii. of the ' Mathematical Monthly') a series of articles " On the Motions of Fluids and Solids relative to the Earth's Surface, comprising Applications to the Winds and the Currents of the Ocean." Mr. Ferrel begins by referring to a pamphlet which he published on the same subject a few years previously, and concludes by pointing out an important modification in one part of his theory as first published, a modification which he has seen it necessary to make after reading the Report of Professor Thomson's paper in the British Association's Proceedings. Mr. Ferrel's paper, as reprinted from the ' Mathematical Monthly,' occupies seventy-two pages, of which about sixty are occupied with an elaborate mathematical investigation of the distribution and motion of the atmosphere which would result from the rotation of the earth combined with the heating of the equatorial regions on the hypothesis of *no friction.* In the latter part of the paper the modifying effects of friction are mentioned, and a theory of general atmospheric circulation, as actually existing, is laid down in such language as to be intelligible to those who are not able or willing to follow the steps of the mathematical investigation.

Without discussing the question of priority, I may say that the views advanced by Mr. Ferrel and by Professor Thomson are substantially the same, as far as they are comparable; but Mr. Ferrel's views are much more fully developed and applied.

As the theory propounded by these two authors (which may appropriately be called the *centrifugal theory of atmospheric distribution and circulation*) is, I believe, very little known among meteorologists, I think I shall be doing good service in calling attention to it, and in pointing out how some of the numerical quantities involved may be calculated without the aid of the higher analysis employed by Mr. Ferrel.

Since the first draft of the present paper was written, a letter has appeared from Mr. Ferrel in ' Nature ' (July 20), in which he calls attention to some of his principal results, and presents some points rather more clearly than in his earlier publications.

that of George Hadley, who published it in the Phil. Trans. for 1735 (vol. xxxix. p. 58). Hadley appears to have been the first to point out the true connexion between the earth's rotation and the easting of the trade-winds. Halley, in 1686 (Phil. Trans. No. 183), had indicated the existence of a circulation of air between the polar and equatorial regions, due to difference of temperature, but he erroneously attributed the easting of the trades to the diurnal wave of heat which runs round the earth from east to west.

To prevent misconception, I should premise that the problem which the theory professes to solve is a broad one. It professes to describe and account for the grand currents of atmospheric circulation over the earth's surface as a whole, and the distribution of the atmosphere in greater or less quantity over the different parallels of latitude, without taking account of local peculiarities depending on the irregular distribution of land and water. *Their* discussion constitutes a separate subject of very great importance ; but as it is easier to conquer difficulties separately than combined, it seems reasonable to begin by neglecting these local complications and treating the earth as a solid of revolution with a uniform surface, especially as we have, in large portions of the existing oceans, a good approximation to these hypothetical conditions, and an opportunity of comparing theory with observation.

The actual state of things over these parts of the ocean may be described by saying that :—

I. As regards wind, there is an equatorial belt of calms, then, on each side of this, a belt some 20° wide covered by the trade-winds, which blow from the east, and at the same time towards the equator, then another calm-belt near the tropic, and then a region, extending as far towards the poles as observations go, over which the prevailing winds are from the west and at the same time towards the pole.

II. As regards quantity of air, the barometer is low in the equatorial calm-belt, from which it gradually rises across the trade-wind region to the tropical calm-belt, where the pressure is greater than on any other part of the earth ; and from this latitude, as far as observations extend, there is a regular fall towards a minimum at each pole, the pressures actually observed in very high latitudes, especially near the South Pole, being very much lower than in any other part of the earth at sea-level.

The theory which I am advocating asserts that these two sets of phenomena stand in the relation of cause and effect. It asserts that the distribution of barometric pressure (in other words, the distribution of the air in greater or less quantity over the different parallels of latitude) is mainly due to the easterly and westerly components of the winds.

It regards the surface of the earth as a surface of equilibrium under the joint action of gravity and the centrifugal force of the earth's rotation. But in the case of a body moving west or east relative to the earth's surface, this equilibrium no longer exists, because the body has greater or less centrifugal force than would be required for equilibrium. West winds consist of air revolving faster than the earth, and there-

fore having an excess of centrifugal force. This excess can be resolved into two components, one of which is vertically upwards, and produces an inappreciable diminution of barometric pressure under it. The other component is directed along the meridian towards the equator, and will produce a deflection towards the equator unless the air which lies on that side has greater elastic force than the air which lies on the polar side. This component, like the vertical one, would be inappreciable if it only acted through a belt five miles wide ; but when the air over a zone many degrees wide has a mean motion from the west, the accumulated effect, in the shape of differential pressure at its two margins, is very considerable.

These remarks respecting the excess of centrifugal force possessed by west winds, apply equally to the defect of centrifugal force in east winds, except that the pressure which they exert in virtue of their relative velocity is directed *from* the equator instead of *towards* it.

Excess and defect of centrifugal force thus produce an increase of barometric pressure from the polar to the tropical limit of the region of west winds, and a similar increase from the equatorial to the tropical limit of the region of east winds. That is to say, the effect due to centrifugal force is everywhere of the same kind as the observed difference*.

As regards the causes of the east and west winds, the prime mover is the increase of temperature from the poles to the equator, which would of itself give a north wind over the whole of the northern hemisphere, and a south wind over the whole of the southern, at the earth's surface, with return-currents in the opposite directions above ; but in virtue of the earth's rotation, which is carrying all points of its surface from west to east with velocities proportional to their distances from the axis, a body set in motion along a meridian towards the equator will fall behind the meridian unless constantly subjected to differential pressure on its two sides ; and a body moving from the equator towards either pole tends, in like manner, to move in advance of the meridian along which it is travelling. Most writers, in

* It is necessary to remark, by way of caution, that the effect in question depends upon the movement of the whole body of air over a region, and not merely of the lower portion whose motion constitutes our observed winds. If some strata are moving to the east and others to the west, opposite signs must be given to eastward and westward velocities, and the average of the whole (not a height-average, but a mass-average) must be struck. We assume that this average velocity is westward in the trade-wind regions and eastward in the temperate zones, thus agreeing with the observed winds. Indeed the fact of a continual circulation of air between the polar and equatorial regions, seems to involve, as a result, that the intertropical parts of the earth's surface are revolving faster, and the other parts of the earth's surface slower than the atmosphere over them.

treating of this subject, express themselves so as to convey the idea that such a body tends to preserve its absolute eastward velocity constant*; but, as Mr. Ferrel has remarked, this is an understatement of the fact ; it is not velocity, but moment of momentum that tends to remain constant, and absolute eastward velocity tends to vary inversely as distance from axis.

The *easting* of the trade-winds is thus clearly accounted for. But the principles thus far adduced would give us in the northern hemisphere only north-west and north-east winds at the surface of the earth, with return-currents from the south above. How is the prevalence of south-west winds at the surface of the earth in the north temperate zone, and of north-west winds in the south temperate zone, to be accounted for ? The account which I believe to be correct, and which in its essential features is due to Professor James Thomson, is as follows :—

If any stratum of air have less than the average eastward or westward velocity (relative to the earth) which prevails through the strata above it, it will not be able to resist the differential pressure from or towards the equator which their motion produces. For this reason the lowest stratum of air, having its velocity relative to the earth kept down by friction, generally moves from the tropical belts of high barometer to the regions of low barometer at the poles and equator. This is the origin of the prevalent winds of the two temperate zones, which must be regarded as constituting under-currents towards the poles, beneath a topmost current, also towards the pole, and a middle return-current.

Between the tropics, on the other hand, the motion thus generated in the lowest stratum of air coincides with the motion due to difference of temperature ; this is probably the reason why the trade-winds are more constant than the winds of the temperate zones.

The easting of winds blowing towards the equator, and the westing of winds blowing towards the pole, may be summed up in the rule that the wind tends to swerve to its right in the northern hemisphere and to its left in the southern : that is to say, in the northern hemisphere, if not constrained by differential pressure, it will blow along a curve concave to its right hand ; and if it is not allowed thus to swerve, but constrained to keep a direct course, it exerts greater pressure against the air on its right hand than against the air on its left. But this tendency is not peculiar to air which is moving in a northward or southward direction. We have already pointed out that air which is moving from west to east over the earth possesses an excess of centrifugal force, in virtue of which it tends to deviate towards

* This mistake occurs in Hadley's original paper.

the equator, while air moving from east to west, relative to the earth, tends to travel towards the nearest pole, so that in both these cases the tendency is to the right in the northern hemisphere and to the left in the southern.   Moreover calculation shows that if the velocity of the wind be small relatively to the absolute rotational velocity of the earth's surface in the neighbourhood, the force necessary to prevent deflection has the same amount in the case of motion along a parallel of latitude, as in the case of motion along a meridian.   Nor is it important, in this respect, to distinguish between motion along a parallel of latitude and motion east or west along an arc of a great circle; for the deflection due to excess or defect of centrifugal force is, in ordinary cases, more than a hundred times greater than the deflection from one of these paths into the other.   For motion along a great circle in any horizontal direction, the intensity of the force necessary to prevent deflection is rigorously the same for all directions, and is

$$2\omega \sin \lambda \cdot v,$$

ω being the angular velocity of the earth's rotation, λ the latitude, and v the velocity of the motion.

The application of these principles to the explanation of cyclones will be at once apparent to those who are familiar with Taylor's explanation as adopted by Dove, Herschel, and other eminent authorities, an explanation founded on the tendency of north and south winds to be deflected in the northern hemisphere to their right and in the southern to their left.   The fact is, that winds from *all* points of the compass, flowing in to a centre of barometric depression, experience this tendency to deflection, and the tendency is the same for them all.   Accordingly, what actually occurs is an inflow from all sides not directly but spirally; or, to put the same fact in other words, there is inflow towards the centre compounded with rotation round it. The rotation is produced and maintained by the pressure which the inflowing components exert each to its own right; and the central depression is maintained by the pressure which the rotating components exert to *their* right, that is, outward from the centre; this is for the northern hemisphere.   For the southern we have only to put *left* for *right*.

The law of oblique inflow from high to low barometer is not confined to storms, but characterizes the ordinary movements of the air, and is manifest on the most cursory glance at the charts of winds and isobaric lines contained in the quarterly publications of the Meteorological Office.   Indeed, judging from these charts, the movement to the right is much more decided than the flow down the barometric gradient.   Buys Ballot's

law, which is a mere summary of observed facts, is stated in the following words in Mr. Buchan's recently published text-book.

"The wind neither blows round the centre of least pressure as circles, or as tangents to the concentric isobaric curves, nor does it blow directly towards that centre; but it takes a direction intermediate, approaching, however, more nearly to the direction and course of the circular curves than of the radii to the centre; or the angle is not a right angle, but from about 60° to 80°."

Dove's law of rotation of wind-direction at any one place (viz. that it changes from north to north-east, then to east, and so round with the hands of a watch in the northern hemisphere, and in the opposite direction in the southern) is probably an indirect consequence of the same tendency to deviate to the right in the one hemisphere and to the left in the other; but Dove's law is not a law in any strict sense of the word, it is merely a statement of what happens in a majority of instances.

The quantitative determination of the forces dealt with in the present paper may be obtained as follows; and it is important to bear in mind that they require no correction for friction.

Let v denote the horizontal velocity of a moving body relative to the earth's surface,

P the constraining force, per unit mass of the body, required to prevent deflection,

λ the latitude in which the body is,

R the earth's radius, about 21 million feet,

ω the earth's angular velocity, which is $\dfrac{2\pi}{86164}$, if the second be the unit of time. This makes $2\omega = \dfrac{1}{6850}$.

I. If the motion be along a meridian, the constraining couple must be equal to the change of angular momentum per unit time; that is,

$$P \cdot R \cos \lambda = \frac{d}{dt} (\omega R^2 \cos^2 \lambda),$$

whence

$$P = -2\omega \sin \lambda \cdot R \frac{d\lambda}{dt},$$

$$= -2\omega \sin \lambda \cdot v.$$

II. If the motion be along a circle of latitude, the excess of the centrifugal force of the moving body above that of a body simply resting on the earth is

$$\frac{(\omega R \cos \lambda + v)^2}{R \cos \lambda} - \frac{(\omega R \cos \lambda)^2}{R \cos \lambda}$$

$$= \frac{2\omega R \cos \lambda \cdot v + v^2}{R \cos \lambda} = 2\omega v \left(1 + \frac{v}{2\omega R \cos \lambda}\right),$$

which is sensibly equal to $2\omega v$, if v be small compared with the absolute eastward velocity of the earth's surface $\omega R \cos \lambda$. The sign of v is positive or negative, according as the relative velocity which it denotes is eastward or westward.

The excess $2\omega v$ may be resolved into $2\omega v \cos \lambda$ vertical, and $2\omega v \sin \lambda$ horizontal, of which the latter must be equal and opposite to the constraining force P.

III. The value of P in both cases (and therefore also for all intermediate directions) is thus $2\omega \sin \lambda \cdot v$, which, if the second be the unit of time, is $\dfrac{v \sin \lambda}{6850}$. It is the same as the constraining force required for motion in a circle of radius $\dfrac{v^2}{P}$ or $\dfrac{v}{2\omega \sin \lambda}$, which, if the second be the unit of time, is $\dfrac{6850 v}{\sin \lambda}$.

IV. Let C A be a small arc of a circle of latitude, B C an arc of a great circle touching it in C, N C and N B meridians. Then N C B is a right-angled spherical triangle, and

$$N A = N C = \frac{\pi}{2} - \lambda.$$

Denote this by α, A B by β, B C by θ. Then, by spherical trigonometry,

$$\cos (\alpha + \beta) = \cos \alpha \cos \theta,$$

or

$$\cos \alpha - \sin \alpha \cdot \beta = \cos \alpha \left(1 - \frac{\theta^2}{2}\right);$$

$\therefore \sin \alpha \cdot \beta = \cos \alpha \cdot \dfrac{\theta^2}{2}$; $\therefore \dfrac{\theta^2}{2\beta} = \tan \alpha = \cotan \lambda$. The radius of the sphere is here the unit of length. In terms of any unit of length, we have $\dfrac{BC^2}{2AB} = R \cotan \lambda$. But B C may be regarded as the tangent to a circular arc A C, and $\dfrac{BC^2}{2AB}$ is the expression for the radius of curvature. The force which would produce deflection from C B into C A, in the case of a body moving with velocity v, is therefore the same as the constraining force required to keep it on the circumference of a circle of radius R $\cotan \lambda$.

V. Since, for given velocity of circular motion, the constraining force varies inversely as the radius, P is to the force which would produce deflection from C B into C A in the ratio

R cotan$\lambda : \dfrac{v}{2\omega \sin\lambda}$, or $\dfrac{2\omega R \cos\lambda}{v}$, which, if the second be the unit of time, and the foot the unit of length, is $\dfrac{3070\cos\lambda}{v}$. In ordinary cases the value of this expression ranges from 100 to several hundreds. The tendency to swerve is therefore sensibly the same for motion along a circle of latitude as for motion along a great circle touching it, in the neighbourhood of the point of contact.

VI. The constraining force on a body moving along a circle of latitude is to the body's weight in the ratio $\dfrac{P}{g}$. If the foot and second be units, g is about 32·2, and $\dfrac{P}{g}$ is $\dfrac{v\sin\lambda}{220{,}000}$. Call this $\dfrac{1}{m}$. Then if the air between two parallels of latitude is moving east or west with velocity v, the change of pressure is the same in going m feet along a meridian as in rising 1 foot, viz. ·00114 inch of mercury. The change of pressure per degree of latitude (365,000 feet), expressed in inches of mercury, is $\dfrac{365000}{m} \times \cdot00114 = \cdot0019\,v\sin\lambda$, v being in feet per second. The average observed difference per degree is about ·01 of an inch. This would require v to be about $\dfrac{5}{\sin\lambda}$ feet per second.

VII. For a cyclone, if r denote distance from its axis, and v the component velocity perpendicular to r, centrifugal force computed as if the earth were at rest gives a barometric difference equivalent to rising a height $\dfrac{1}{g}\displaystyle\int \dfrac{v^2}{r}\,dr$.

The earth's rotation adds to this a difference equivalent to rising a height $\displaystyle\int \dfrac{dr}{m}$.

VIII. The following investigation can be employed instead of I., II., III. The earth's rotation ω may be resolved into a translation, a rotation about a horizontal axis (at the place considered), and a rotation about a vertical axis. The two former may be neglected. The third is $\omega\sin\lambda$, which call $\omega_{\prime}$. The lateral constraining force is therefore the same for a body moving horizontally in a straight line or in a great circle, as for a body travelling along a radius of a horizontal disk revolving about its centre with angular velocity $\omega_{\prime}$. Let r denote distance from

centre of disk, then we have

$$\text{P}r = \frac{d}{dt}\,(\omega_i r^2) = 2\omega_i r\,\frac{dr}{dt} = 2\omega_i r v\,;$$

$$\therefore\ \text{P} = 2\omega_i v = 2\omega \sin \lambda\,.\,v.$$

Hence, for horizontal motion in a great circle, the tendency to swerve is rigorously equal for all directions of motion, a result which will be found to agree with the comparison of II. and V.

XXV. *Account of Experiments upon the Resistance of Air to the Motion of Vortex-rings. By* ROBERT STAWELL BALL, *A.M., Professor of Applied Mathematics and Mechanism, Royal College of Science for Ireland, Dublin*.*

THE experiments, of which the following is an abstract, were carried out with the aid of a grant from the Royal Irish Academy. A paper containing the results has been laid before the Academy. A brief account of one series of the experiments and a Table embodying them will be given.

Air-rings 9 inches in diameter were projected from a cubical box, each edge of which is 2 feet†. The blows were delivered by means of a pendulum called the striker, which, falling from a constant height, ensured that the rings were projected with a constant velocity. In the experiments described in the present series, this velocity was a little over 10 feet per second. The pendulum was arranged so as to open a circuit at the instant of its release.

After the ring had traversed a range which varied from 2 inches to 20 feet, it impinged upon a target. The blow upon the target closed the circuit, which had been opened at the release of the striker. A chronoscope measured the interval of time between the release of the striker and the impact upon the target.

The target was placed successively at distances of 2, 4, 6, 8, 10, 12, 14, 16, 18, 20 feet from the orifice of the box. Not less than ten observations of the time were taken at each range. The probable error of the mean time at each range is in every case less than 1 per cent. of the whole amount. A special series of experiments, which need not be described, determined the value of the chronoscope readings in seconds.

The observations are next represented in a curve, of which the abscissæ are the ranges, and the ordinates the corresponding mean chronoscope readings. By drawing tangents to this curve,

* Communicated by the Author, being an abstract of a paper read before the Royal Irish Academy.

† This method was suggested by Professor Tait. See a paper by Sir William Thomson, Phil. Mag. July 1867; also a paper by the author, Phil. Mag. July 1868.

the velocity of the ring at its different points is approximately found.

A second projection is made in which the abscissæ are the ranges and the ordinates are the velocities; the points thus determined are approximately in a straight line.

It follows that the rings are retarded as if acted upon by a force proportional to the velocity, and an approximate value of the numerical coefficient becomes known.

A more accurate value having been determined by the method of least squares, the results are embodied in the following Table, of which a description is first given.

I. contains a series of numbers for convenience of reference.

II. It was found that the motion of the ring in the immediate vicinity of the box was influenced by some disturbing element. The zero of range was therefore taken at a point 4 feet distant from the orifice. This column contains the ranges.

III. The interval between the release of the striker and the arrival of the ring at a point 4 feet from the orifice is 6·5 chronoscopic units, or about 0·93 second. This constant must be subtracted from the mean readings of the time in order to reduce the zero epoch to the instant when the ring is 4 feet from the orifice. This column contains the mean readings of the chronoscope corrected by this amount.

IV. When the ranges are taken as abscissæ and the corresponding times as ordinates, it is found that a curve can be drawn through or near all the points thus produced. To identify the points with the curve, small corrections are in some cases required. These corrections are shown in column IV. In the case of No. 5 the correction amounts to 0·7. This is about 0·09 second. The magnitude of this error appears to show that some derangement, owing perhaps to a current of air or other source of irregularity, has vitiated this result. For the sake of uniformity, however, the corrected value has been retained.

V. This column merely contains the corrected means, as read off upon the curve determined by the points.

VI. The value of the chronoscope unit after the first few revolutions is

$$0{\cdot}1288 \text{ second,}$$

with a probable error of 0·0002 second.

By means of this factor the corrected means in column V. are evaluated in seconds in column VI.

VII. This column contains the time calculated on the hypothesis that the rings are retarded as if acted upon by a force proportional to the velocity, the coefficients being determined by the method of least squares; the formula is

$$t = 9{\cdot}016 - 6{\cdot}25 \log (27{\cdot}7 - s).$$

VIII. This column shows that the difference between the corrected mean time and the calculated time in no case exceeds

0·01 second.

IX. The approximate velocities deduced by drawing tangents to the curve.

X. The true velocities calculated from the formula

$$\frac{ds}{dt} = 0·368(27·7 - s).$$

XI. The retarding force,

$$-\frac{d^2s}{dt^2} = 0·136(27·7 - s).$$

TABLE of Experiments showing the retardation which a Vortex-ring of air experiences when moving through air at the same temperature and pressure. The vortex-ring is 9 inches in diameter, and has an initial velocity of 10·2 feet per second. The retarding force is proportional to the velocity; and after 2·34 seconds the ring has moved 16 feet, and its velocity is reduced to 4·3 feet per second.

I. Reference number.	II. Distance of target from orifice, minus 4 feet. s.	III. Mean corresponding reading of chronoscope, minus 6·5.	IV. Correction deduced by graphical construction.	V. Corrected value of mean chronoscopic reading.	VI. Equivalent time, in seconds.
1.	2 feet	1·6	0·0	1·6	0·21
2.	4 ,,	3·4	−0·1	3·3	0·43
3.	6 ,,	5·4	−0·2	5·2	0·67
4.	8 ,,	7·3	0·0	7·3	0·93
5.	10 ,,	10·2	−0·7 !	9·5	1·23
6.	12 ,,	12·0	−0·1	11·9	1·54
7.	14 ,,	14·5	+0·3	14·8	1·91
8.	16 ,,	18·2	−0·1	18·1	2·33

Reference number.	VII. Time calculated by formula $t = 9·016 - 6·25 \log (27·7 - s)$.	VIII. Difference between calculated and observed time.	IX. Approximate velocity deduced from graphical construction.	X. True velocity, calculated by $\frac{ds}{dt} = 0·368(27·7 - s)$.	XI. Retarding force, calculated by $-\frac{d^2s}{dt^2} = 0·136(27·7 - s)$.
1.	0·20	−0·01	9·3	9·5	3·5
2.	0·42	−0·01	8·4	8·7	3·2
3.	0·66	−0·01	7·7	8·0	3·0
5.	0·92	−0·01	7·3	7·2	2·7
5.	1·22	−0·01	6·4	6·5	2·4
6.	1·54	0·00	5·9	5·8	2·1
7.	1·91	0·00	5·2	5·0	1·9
8.	2·34	+0·01	4·4	4·3	1·6

XXVI. *On the Action of Magnetism on Gases traversed by Electric Discharges. By* MM. A. DE LA RIVE *and* E. SARASIN*.

ONE of us (M. de la Rive) has been for a long time occupied with the action of magnetism on the electric jets which are propagated in very rarefied gaseous media. In his last researches, which appeared in the *Archives des Sciences Physiques†*, M. de la Rive showed that the action of magnetism determines a considerable increase of the resistance to electric conduction— an increase which varies according to the portion of the jet submitted to the action of the electromagnet, and according to the position of the tube traversed by the discharge relatively to the magnetic poles.

M. de la Rive has also studied in detail the rotation effects produced by the action of one magnetic pole on the electric jets in various much rarefied gaseous media. He has described the differences of velocity which result from the greater or less degree of rarefaction of the medium and from its greater or less conductivity, dwelling particularly on the curious appearance presented by the electric jet in a medium containing a rather considerable proportion of aqueous or alcoholic vapour—which consists in the jet being divided into several, forming as it were the spokes of a wheel, and which never takes place at any degree of rarefaction in gaseous media which contain no vapour.

At the end of his work M. de la Rive intimated that there were still many points in this interesting subject which it was important to elucidate. This is the study which we have undertaken together, and the results of which we now communicate to the Society.

The first point we had to ascertain was, whether, when an electric jet traverses a rarefied gaseous medium, the influence of the magnetism determines a change of density, probably an increase, in the portion of the gas submitted to the action of the magnet.

We next sought to determine the influence of magnetism on the electric conductivity of the rarefied gases traversed by the discharge when this is effected in a direction perpendicular to the line joining the poles of the electromagnet—that is to say, equatorially.

Then we studied this influence in a case in which it had not been previously studied, viz. when the discharge is effected axially, or along the line which joins the two poles.

Finally we examined in the same connexion a third case, in which the electric jet is caused to rotate continuously under the action of an electromagnet arranged for that purpose within the rarefied gaseous medium.

As our aim was not to search out for each gas the numerical coefficients relative to those different kinds of action, but only to determine the general laws of the phenomena with which we are engaged, we have limited our operations to three gases very different in their physical and chemical properties, viz. *atmospheric air*, *hydrogen*, and *carbonic acid*.

I. *Influence of Magnetism on the Density of the rarefied Gas traversed by the Electric Discharge.*

When the electric jet transmitted through a rarefied gas in a tube is submitted to the action of magnetism, the jet, tending, as Plücker has shown, to describe a magnetic curve, bears towards the sides of the tube (which just prevent it from describing exactly the curve), and that to one side or the other, according to the relative direction of the magnetism and of the electric current. At the same time the jet, which spreads itself out more or less in the tube according to the density of the gas, seems to contract to a tolerably thin thread. One is therefore disposed to believe that the gas itself is condensed under the action of the electromagnet, and that perhaps this is the cause to which is due the greater resistance it then offers to electric conduction ; consequently we thought we ought to commence our work by endeavouring to ascertain if this condensation really takes place.

From the first trials we made, by putting the tube traversed by the electric discharge under the action of magnetism into communication with a very sensitive manometer, we recognized that the apparent condensation of the electric jet is accompanied by an augmentation of the elastic force of the gas ; and this fact is observed with the greatest facility. When the induction-current is passed through a tube filled with any gas brought to a certain degree of rarefaction, and communicating with the manometer, the latter indicates a very notable increase of pressure at the moment the discharge begins to pass. This increase of pressure evidently results from the heating of the gas. When the tube, continuously traversed by the electric discharge, is afterwards submitted to the action of magnetism, the pressure in the tube is seen to diminish, but never returns quite to what it was before the passage of the current. Under the action of the magnetization, then, the elastic force of the gas traversed by the electric discharge has been sensibly diminished, though without returning to what it was before the passage of the current. This diminution must be due, at least in part, to the weakening of the current, which from that time must produce less heating.

In order to study this special point more closely, we made use of a large glass tube with two compartments divided the one from the other by a stopcock, also of glass, with a large opening. The length of this tube is 51 centimetres, its diameter 65 millimetres, the length of each of the two compartments 225 millims.; they are joined together by a tubulure 60 millims. long., 10 millims. in diameter; and the opening of the glass stopcock has the same section as the latter. The stopcock, working with perfect smoothness, closes hermetically. The tube carries at its two extremities brass mountings, with stopcocks and knobbed electrodes, also of brass. The distance from one electrode to the other is 41 centims. The tube is placed transversely between the two poles of the electromagnet in such a manner that the centre of one of the compartments is on the axis of the magnet, while the other is completely withdrawn from the action of the magnetism. The electromagnet used in these experiments was that described by M. de la Rive in his memoir on the magnetic rotatory polarization of liquids*. We usually magnetized it with the current produced by 40 Bunsen couples. In order that the magnetic action might be as intense as possible on one of the two moieties of the electric discharge, the two poles of the electromagnet were brought into immediate contact with the sides of that compartment of the glass tube which was to be submitted to its action. This compartment was put in communication, by means of a small leaden tube, with a very sensitive manometer, consisting of a double barometer and a cathetometer which permitted the estimation of hundredths of a millimetre. The other compartment was connected by a system of leaden tubes with an ordinary air-pump, a mercurial air-pump, and an apparatus for introducing and desiccating the gases. The induced current was furnished by a Ruhmkorff coil of medium size excited by the current of a Grove pile of four pairs.

In making the experiment, we commenced by producing a vacuum throughout the apparatus, then introduced the gas which was to be operated on, again exhausted the apparatus, reintroduced the gas, repeating the process several times, till the contained gas was sufficiently pure; the gas in the tube was then brought to a well-determined pressure, which could be easily done, within 5 or 10 hundredths of a millimetre, either with the mercurial pump, or with the ordinary air-pump, by suitably regulating the working of the stopcocks.

Communication with the lead pipes being closed, the level of the mercury is taken very exactly; then the cock which regulates the communication between the glass tube and the mano-

* *Archives des Sci. Phys. et Nat.* vol. xxxviii. p. 209; Phil. Mag. S. 4. vol. xl. p. 394.

meter is closed, the electromagnet magnetized, and the current passed. In the compartment which is placed between the two magnetic poles the luminous jet is very much condensed and driven against the side of the tube. In the other compartment scarcely any effect is apparent, except the diminution of brightness resulting from the considerable augmentation of resistance produced in the compartment submitted to magnetization, and consequently the diminution of the intensity of the discharge. The magnetism is permitted to act during from 10 to 20 seconds; then the large glass cock is closed, while the current is still propagating; at the same moment the interruptor of the Ruhmkorff is turned, and thus the current is stopped. Finally, the manometer is observed while the cock which separates the first compartment from the manometer is opened and then the large glass cock. By operating thus it is found that the gas contained in the compartment on which the magnet has acted presents a very sensible increase of pressure, while in the other an equivalent partial vacuum has been produced. By observing the manometer, a great oscillation is seen to be produced there the moment the cock is opened which puts it in communication with the compartment on which the magnet has acted; the level of the mercury falls several hundredths of a millimetre. Then, when we open the glass cock (which puts the second compartment in communication with the first and with the manometer), we observe a second oscillation, in direction opposite to the first; and when the motion of the mercury has ceased, we see that its level has returned exactly to what it was before the experiment. Thus, then, under the action of the magnetism, a certain quantity of gas has passed from the compartment which is outside of that action into that which was submitted to it, and has consequently increased the density of the gas contained therein.

This effect, of course, varies with the force of the magnet, with the intensity of the induced current, and the initial pressure of the gas operated on. It evidently increases with the intensity of the magnetism and with that of the discharge; it increases also very notably with the initial density of the gas, provided the discharge is strong enough for its intensity not to be too much weakened by this augmentation of density. For a given intensity of the current of the induction-coil, and a determined distance of the electrodes, there is, then, for each gas a pressure at which the effect observed is the maximum. This pressure is the lowest for air (the worst conductor of the three gases on which we operated), high for carbonic acid, and higher still for hydrogen. For example, in the large tube with the glass cock, and with the Ruhmkorff of medium size, the three pressures corresponding to the maximum effect were 7–8 millims. for air,

10–12 millims. for carbonic acid, and 15 millims. for hydrogen. A very great number of experiments have shown us that the condensation·is the greatest with air, sensibly less with carbonic acid, and very slight with hydrogen. With the medium-sized Ruhmkorff coil, we obtained for the variation of the elastic force, having for the three gases the same initial pressure of 8 millims. :—

Air.	Carbonic acid.	Hydrogen.
millim. millim.	millim. millim.	millim. millim.
0·12 and 0·16	0·08 and 0·12	0·02 and 0·04.

It was needless, as may be seen, to reduce the current to the same intensity in each case, as we had done at first; for even when this is not done the order in which the gases range themselves does not change; and the result is then so much the more significant, because the intensity of the current, greater in carbonic acid and hydrogen than in air, ought on the contrary, if it disturbed the experiments, to invert the order of the three gases.

Although the numerous experiments made under these conditions left no doubt of the effect, variable from one gas to the other, which magnetism exerts on a rarefied medium traversed by the electric discharge, we desired to resume the experiments, and to repeat them with a very powerful Ruhmkorff coil, which permitted us to work with much higher pressures by having a stronger current, and to obtain thereby even much more pronounced effects. The following Table contains the results obtained in this new series of experiments :—

Differences of Pressure observed.

	Air.	Carbonic acid.	Hydrogen.
millim.	millim. millim.	millim. millim.	millim.
At 8	0·10 to 0·12	0·04 to 0·06	0·02
15	0·24 to 0·28	0·16	0·04
20	0·32 to 0·36	0·20	0·06

These numbers refer to the case in which the compartment containing the negative electrode is placed between the two poles of the magnet, the other being placed outside of its action. Our experiments showed that the effect is much less in the inverse case. When the magnet acted on the positive part of the jet, we obtained for that variation of pressure with air, at 20 millims., 0·16–0·18 millim. instead of 0·32 millim.

It results, then, from these experiments that the action of magnetism sensibly increases the density of a gaseous medium traversed by the electric discharge in that portion of the medium

on which the action is exerted. This effect varies according to the different gases : it is so much the greater as the electric conducting-power of the gas is less. It varies also according to the portion of the jet submitted to the action of the magnet, being a maximum when the magnet acts on the negative portion.

We confine ourselves for the present to stating the fact, without venturing as yet to give the interpretation of it. It may proceed either from a real condensation of the gaseous molecules effected by the magnetism, or from a difference of temperature between the part of the jet which is submitted to the magnetic action and that which is not, or from both causes combined. We purpose to resume the examination of this question in the study which we intend to make of the influence of magnetism on the calorific effects produced by the passage of the electric discharge through rarefied gases.

II. *Action of Magnetism on the Conductivity of Rarefied Gases when the Electric Discharge is directed transversely to the line which joins the poles of the electromagnet, or Equatorially.*

In order to study the influence of magnetism on the resistance opposed by a rarefied gas to the transmission of the electric discharge when this takes place in a direction perpendicular to the line of the poles of the electromagnet, we used a cylindrical glass tube of which the dimensions were such that the electric jet was submitted in its whole length to the intense action of the magnetism. This tube was 20 centims. long, and 35 millims. in diameter; at each extremity it had a brass mounting with a stopcock and a knobbed electrode, also of brass. The distance between the two electrodes was equal to the diameter of the soft iron of the electromagnet, viz. 9 centims. This cylinder was arranged like the large tube with glass stopcock in the preceding experiment, and communicated by lead pipes, at one end with the manometer, and at the other with the air-pump.

That we might be able to institute a comparison between the three gases on which we wished to operate, it was necessary to reduce them all to the same pressure, and pass through them a current of the same intensity. But as it was not easy to place a rheostat in the induced circuit, we thought of varying the intensity of the inducing current by introducing a wire of German silver of greater or less length. The induced current traversing the rarefied gas was measured by means of a very sensitive galvanometer* and the apparatus described by M. de la Rive in his researches on the electric discharge in rarefied gases†. Lastly,

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* This galvanometer was placed in an adjoining room, at a sufficient distance from the electromagnet to preclude its needle being influenced by it.

† *Archives des Sciences Phys. et Nat.* July 1860, vol. xxvi. p. 177.

as the gaseous column on which we operated was very short and not sufficient, especially in the case of hydrogen, to eliminate the induced current of feeble tension given by the Ruhmkorff coil at the same time as the current of strong tension, in the opposite direction, which was used in the experiment, we introduced into the induced circuit a cylindrical Geissler tube of 49 centims. length, 30 millims. diameter, and containing rarefied hydrogen.

To measure the influence of magnetism on the electric conduc- tivity of a rarefied gas traversed by the induction-current, we reduced each of the gases operated on to a given pressure, the same for all three; then we regulated the intensity of the indu- cing current in such manner as to have always, in each series of experiments, the same intensity of the induced current. Having thus, at the commencement of each experiment, placed the gas in identical initial conditions, we magnetized; then we sought the degree of rarefaction to which it was necessary to reduce it in order that, under the action of the magnetism, the intensity of the traversing current might become the same as before the magnetization. This method is more delicate than that which would consist simply in measuring the diminution produced in the deflection of the galvanometer when the rarefied gas is sub- mitted to the action of magnetism. That deflection may, in fact, under the action of magnetism, be reduced from 60° or more to a very slight one—from 6° to 8°, for example, and even less, about the value of which it is very easy to make a mistake. There is, besides, a great advantage in varying the pressure, of which a very precise measurement can be obtained—rather than the intensity, which cannot be accurately measured by the de- flections of the galvanometer-needle, to which it is not propor- tional when they exceed 30°.

We commenced the experiment by making a vacuum through- out the system; then we introduced dry and pure the gas to be operated on, exhausted again, reintroduced the gas, and repeated this two or three times in succession, so as to expel foreign gases. By afterwards suitably regulating the cocks of the air-pump, we succeeded in making its action perfectly regular, and thus ob- tained, very exactly, the degree of rarefaction desired (30 mil- lims. for example). The moment this pressure was reached, the cock of the glass tube was closed; then the intensity of the in- ducing current was regulated so as to obtain the required deflec- tion of the galvanometer. This being done, we magnetized, and gradually exhausted the tube until the galvanometer-deflection had returned to exactly what it was before. We then again in- terrupted the communication of the glass tube with the pump, verified again that the induced current had really resumed pre- cisely its initial intensity, and, lastly, took the reading of the

manometer, which during the whole experiment had remained in communication with the glass tube.

After repeating these experiments a few times, one can make them with great rapidity and yet obtain, for the same gas in the same conditions, almost absolutely concordant results.

We executed several series of comparative experiments with air, hydrogen, and carbonic acid, at pressures of 20, 30, and 40 millims. We limit ourselves to giving the results of some; all, however, were perfectly concordant with one another.

Atmospheric air.		Hydrogen.	
Defl. of galv.	Pressure. millims.	Defl. of galv.	Pressure. millims.
30° without magnetizing	20	30° without magnetizing	20
with magnetization	6·64	with magnetization	3·50
	6·56		2·90
	6·20		3·20
	6·58	Mean . .	3·20
Mean . .	6·50		

From this Table is seen how the influence of magnetism on the electric conductivity of a gas traversed by the induction-current varies from one gas to the other. The numbers just given show that magnetism diminishes the electric conductivity of hydrogen much more than that of air; yet the apparent contraction of the jet under the action of magnetism is much less in hydrogen than in air. This at once makes it evident that the condensation of the jet is not the sole cause of the augmentation of resistance, and that probably it is not even the preponderant cause.

The following are the results obtained in a series chosen from several others which we made on the three gases in question:—

Initial pressure of the three gases . . 30 millims.
Constant deflection of the galvanometer. 30°

Pressure under the action of magnetization:—

	Hydrogen.	Carbonic acid.	Air.
	millims.	millims.	millims.
	7·30	10·60	13·70
	7·60	10·42	13·34
	7·60	10·55	—
Mean .	7·50	10·52	13·52

The same result at 20 millims:

Initial pressure of the three gases . . 20 millims.
Constant deflection of the galvanometer. 30°

Pressure under the action of magnetization :—

	Hydrogen.	Carbonic acid.	Air.
	millims.	millims.	millims.
	2·45	3·20	6·72
	2·55	3·30	6·52
	2·80	—	6·50
Mean .	2·60	3·25	6·58

The effect was the same also at 40 millims.; only we could not obtain very regular results with air at that pressure, the discharge therein being too discontinuous.

Initial pressure of the two gases . . 40 millims.
Constant deflection of the galvanometer. 30°

Pressure under the action of magnetization :—

	Hydrogen.	Carbonic acid.
	millims.	millims.
	13·54	16·00
	13·60	16·24
Mean . .	13·57	16·12

From these multiplied experiments we may therefore conclude that the increase of resistance which results for a gas from the action of magnetism is as much greater as the electric conducting-power of the gas is greater. In fact we have seen that it is greatest for hydrogen, the conducting-power of which is very great—less for carbonic acid, which is sensibly less conductive—and least for atmospheric air, which presents a much greater resistance than the two preceding gases. As we have already remarked, the compressibility appears to play only a very secondary part, if any, in this class of phenomena.

III. *Action of Magnetism on the Electric Jet when this is directed along the line which joins the magnetic poles, or Axially.*

The action of magnetism on the electric jet varies, of course, much with the position occupied by the jet relative to the poles of the electromagnet. Divers physicists have occupied themselves with this subject, and in their researches have considered the most varied cases; but we do not think that the simple case which forms the subject of this section, and, as we shall see, is very interesting, has yet been particularly studied.

It was necessary that the two ends of the tube through which we transmitted the electric discharge should be capable of being introduced into the cylindrical opening in the two pieces of soft iron of the electromagnet. Therefore the apparatus we used in this series of experiments consisted of a narrow glass tube, 22

millims. in internal diameter, and 40 centims. long ; the jet had a length of 20 centims.   To the two extremities of this tube were cemented the two electrodes, each formed of a brass rod ending in a knob, likewise of brass.   One of these electrodes extended externally in a long brass stem ending at the extremity of the opening in the soft iron, the other in a lead pipe very narrow and flexible, which, passing through the opening in the second soft iron, put the glass tube in communication with the manometer and the air-pump.   The effect of magnetism on the electric discharge thus arranged axially varies with the distance of the poles of the magnet.   With our apparatus, the most favourable case appeared to be that in which we had a fixed distance of 10 centims. between the two poles, which distance we maintained throughout our experiments.   The effect of magnetism on the resistance of the rarefied gas contained in the axial tube, as well as on the appearance of the electric jet, varies likewise with the position occupied by the electrodes in relation to the magnetic poles ; the action of magnetism on the resistance reaches its maximum when the knob serving as the negative electrode is in the middle of the interval separating the magnetic poles.   The influence of magnetism on the axial discharge is very feeble at pressures above 2 millims.   Nevertheless, even in this case the magnetization sensibly modifies the appearance of the jet : having been discontinuous and striated, it becomes much more continuous; still it is not possible as yet to verify an appreciable change in the resistance.   From 2 millims. the effect is much more pronounced ; and here we must distinguish two principal positions of the electrodes in the tube.

1_st position._ The negative electrode is placed in the centre of the interval separating the magnetic poles.   When the gas is brought to a pressure below 2 millims., the moment we magnetize, a complete modification in the appearance of the discharge is observed.   The extremity of the positive jet lengthens into a dart which applies itself to the side of the tube, approaching more and more to the negative electrode, and ends, at the lowest pressures we have been able to attain, by passing between the tube and the negative knob to unite behind it with a blue sheath which has replaced the negative aureole.   The change produced in the resistance is still more remarkable than the modification in the appearance of the jet : indeed, in this case, contrary to what would take place with the transverse discharge, the electric conductivity of the gas traversed by the electric jet is _increased_ under the influence of magnetism.   At 1 millim. pressure, for example, while the galvanometer placed in the derived current gave a deflection of $30°$ when the electromagnet was not in action, it marked $35°$ after the magnetization in the case of air, $38°$ in

the case of carbonic acid, and 40° in the case of hydrogen. We see by this that the electric conductivity of the rarefied gas contained in the axial tube is notably augmented by the magnetizing. At the same time we ascertain that the effect is not the same in the different gases : it is maximum in hydrogen, minimum in air ; the order of the three gases is the same here as in the experiments with the transverse discharge. The less the pressure, the more marked is the effect ; at $\frac{1}{2}$ millim. pressure we have seen the deflection of the galvanometer pass, in consequence of the magnetizing, from 15° to 26° with air, to 30° with carbonic acid, and to 38° with hydrogen : it is seen that the intensity of the current was more than doubled in hydrogen.

2nd position. The negative electrode is in the immediate vicinity of one of the magnetic poles. The modification induced in this case in the appearance of the electric jet is much more remarkable than in the preceding. In proportion as the pressure diminishes below 2 millims., the luminous envelope which surrounded the negative electrode lengthens more and more, invading the dark space ; at the lowest pressures we have been able to attain, the negative part of the jet formed at last a very elongated frustum of a cone, filling the whole interval between the two electrodes, the positive part having been driven back into the interior of the soft-iron cylinder. In this position of the tube the increase observed in the conductivity of the rarefied gas is a little less than that obtained in the first position. By slowly moving the tube, it may be made to take all the positions between the two on which we have particularly dwelt. Starting from the second position, we see the negative luminous cone shorten more and more, then give place to the positive dart, which advances as far as behind the negative electrode. The direction of the magnetization has no influence, either on the increase of conductivity or on the appearance of the electric jet.

M. de la Rive had already, in his study on the same subject (cited at the beginning of our memoir), described a special case in which the action of magnetism appeared to him to increase the electric conductivity of the gas instead of diminishing it : it was where the discharge was transmitted through a spiral tube placed in a peculiar manner between the poles of the electromagnet ; but he did not dwell on it, reserving the study of it for a future memoir[*].

IV. *The Action of Magnetism on the Electric Jet when this is rotated continuously round the pole of the Electromagnet.*

The rotatory movement of the jet may either be performed in a plane perpendicular to the axis of the electromagnet which

* *Archives des Sci. Phys. et Nat.* Dec. 1866, vol. xxvii. p. 296.

produces the rotation (which takes place when the spark strikes
between a metallic ring perpendicular to the axis of the magnet
and an electrode placed in its centre in the continuation of that
axis*), or take place vertically round the axis of a small cylinder
of soft iron magnetized by contact with one of the poles of the
electromagnet, and of which the extremity constituted one of the
electrodes.

In the first case there is no sensible variation of conductivity
in the gas when the jet is put in movement by the action of
magnetism ; the conductivity remains exactly what it was when
the jet was not under the influence of the magnet and conse-
quently was at rest.   It is the same when, aqueous or alcoholic
vapour being introduced into the rarefied gas, under the mag-
netic influence the jet, previously single, is divided into several
resembling the spokes of a wheel.

With aqueous vapour the medium at the same degree of ten-
sion is more conductive than the dry gas ; with the vapour of
alcohol it is less so, in the same conditions ; but with neither of
these two vapours, any more than with the dry gas, does the
magnetism, when it determines the rotation of the jet, influence
the electric conductivity.

It is quite different when the jet describes round the rod of
magnetized soft iron a cylinder whose axis is that of the rod. In
this case there is a very sensible increase of resistance to the elec-
tric conduction when the jet, instead of being motionless, rotates
from the effect of the magnetism.   But this increase is sensibly
greater when it is positive electricity that issues from the apex
of the soft iron than when it is negative.   Thus in the first case
we have seen the deflection of the galvanometer diminish from
65° to 45°, while in the second case it diminished only from 65°
to 55°.   Let us remark that in the case in which the diminution
of conductivity is the greatest the rotation of the jet seems to
be effected with more difficulty, in the same conditions of inten-
sity of the electric discharge, intensity of magnetism, and rarefac-
tion of the gaseous medium, which is simply atmospheric air at
4 millims. pressure : not only is the rotation much less rapid, but
the jet itself, instead of remaining vertical, takes during its rota-
tion an inclined position ; this is observed to a certain degree in
the other case, but is much more pronounced in that in which
the conductivity is most diminished.

It would therefore seem that this diminution of conductivity
corresponds to the constrained position which the electric jet is
forced to take under the influence of the magnetizing in the case
in which it is naturally vertical ; whereas, when it is naturally

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* In these experiments the magnet was arranged as a vertical column,
instead of in a horseshoe-form as in the preceding experiments.

horizontal and turns in the same direction as the hands of a watch, the magnetism merely impresses on it a continuous movement of rotation, without making any alteration in its form, its direction, or its appearance.

Conclusions.

It follows from the experiments described in this memoir :—

1. That the action of magnetism, when it is exerted only on a portion of an electric jet transmitted through a rarefied gas, determines in that portion an increase of density.

2. That the same action, when it is exerted on an electric jet placed *equatorially* between the poles of an electromagnet, produces in the rarefied gas in which it is propagated *an increase* of resistance which is as much greater as the gas itself is more conductive.

3. That this action, on the contrary, determines a *diminution* of resistance when the jet is directed *axially* between the two magnetic poles, this diminution being as much greater as the gas is more conductive.

4. That when the action of the magnetism consists in impressing a continuous movement of rotation on the electric jet, it has no influence on the resistance to conduction, if the rotation is effected in a plane perpendicular to the axis of the magnetized soft-iron cylinder which determines the rotation; while it notably diminishes it if the rotation takes place so that the electric jet describes a cylinder round the axis of the rod.

5. That these different effects apparently cannot be attributed to variations of density produced in the gaseous medium by the magnetic action, but very probably their explanation will be found in the perturbation induced by that action in the arrangement (or disposition of the particles of the rarefied gas) necessary for the propagation of electricity.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 25, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communication was read :—

"Note on the Spectrum of Uranus and the Spectrum of Comet I., 1871." By William Huggins, LL.D., D.C.L., V.P.R.S.

In the paper "On the Spectra of some of the Fixed Stars"*, presented conjointly by Dr. Miller and myself to the Royal Society

* Phil. Trans. 1864, p. 413; and for Mars, Monthly Notices R. Astr. Soc. vol. xxvii. p. 178.

in 1864, we gave the results of our observations of the spectra of the planets Venus, Mars, Jupiter, and Saturn ; but we found the light from Uranus and Neptune too faint to be satisfactorily examined with the spectroscope.

By means of the equatorial refractor of 15 inches aperture, by Messrs. Grubb and Son, recently placed in my hands by the Royal Society, I have succeeded in making the observations described in this paper of the remarkable spectrum which is afforded by the light of the planet Uranus.

It should be stated that the spectrum of Uranus was observed by Father Secchi in 1869[*]. He says, "le jaune y fait complétement défaut. Dans le vert et dans le bleu il y a deux raies très-larges et très-noires." He represents the band in the blue as more refrangible than F, and the one in the green as near E.
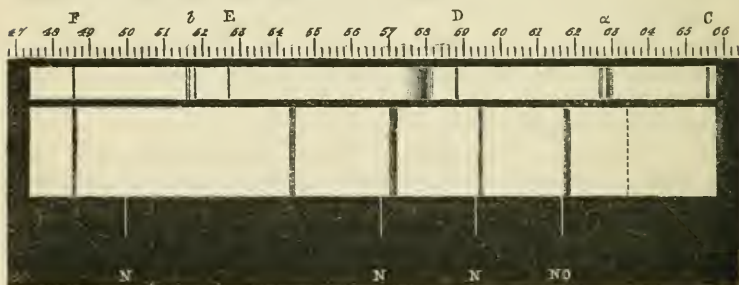
The spectrum of Uranus, as it appears in my instrument, is represented in the accompanying diagram. The narrow spectrum placed above that of Uranus gives the relative positions of the principal solar lines and of the two strongest absorption-bands produced by our atmosphere—namely, the group of lines a little more refrangible than D, and the group which occurs about midway from C to D. The scale placed above gives wave-lengths in millionths of a millimetre.

The spectrum of Uranus is continuous, without any part being wanting, as far as the feebleness of its light permits it to be traced, which is from about C to about G.

On account of the small amount of light received from this planet, I was not able to use a slit sufficiently narrow to bring out the Fraunhofer lines. The positions of the bands produced by planetary absorption, which are broad and strong in comparison with the solar lines, were determined by the micrometer and by direct comparison with the spectra of terrestrial substances.

The spectroscope was furnished with one prism of dense flint-glass, having a refracting-angle of 60°, an observing telescope magnifying $5\frac{1}{2}$ diameters, and a collimator of 5 inches focal length. A cylindrical lens was used to increase the breadth of the spectrum.

The remarkable absorption taking place at Uranus shows itself in six strong lines, which are drawn in the diagram. The least

* Comptes Rendus, vol. lxviii. p. 761, and 'Le Soleil,' Paris, 1870, p. 354.

refrangible of these lines occurs in a faint part of the spectrum, and could not be measured. Its position was estimated only, and on this account it is represented in the diagram by a dotted line. The positions of the other lines were obtained by micrometrical measures on different nights. The strongest of the lines is that which has a wave-length of about 544 millionths of a millimetre. The band at 572 of the scale is nearly as broad but not so dark; the one a little less refrangible than D is narrower than the others.

The measures taken of the most refrangible band showed that it was at or very near the position of F in the solar spectrum. The light from a tube containing rarefied hydrogen, rendered luminous by the induction-spark, was then compared directly with that of Uranus. The band in the planet's spectrum appeared to be coincident with the bright line of hydrogen.

Three of the bands were shown by the micrometer not to differ greatly in position from some of the bright lines of the spectrum of air. A direct comparison was made, when the principal bright lines were found to have the positions, relatively to the lines of planetary absorption, which are shown in the diagram. The band which has a wave-length of about 572 millionths of a millimetre is less refrangible than the double line of nitrogen which occurs near it. The two planetary bands at 595 and 618 of the scale appeared very nearly coincident with bright lines of air. The faintness of the planet's spectrum did not admit of certainty on this point; I suspected that the planetary lines are in a small degree less refrangible. There is no strong line in the spectrum of Uranus in the position of the strongest of the lines of air, namely the double line of nitrogen.

As carbonic acid gas might be considered, without much improbability, to be a constituent of the atmosphere of Uranus, I took measures with the same spectroscope of the principal groups of bright lines which present themselves when the induction-spark is passed through this gas. The result was to show that the bands of Uranus cannot be ascribed to the absorption of this gas.

There is no absorption-band at the position of the line of sodium. It will be seen by a reference to the diagram that there are no lines in the spectrum of Uranus at the positions of the principal groups produced by the absorption of the earth's atmosphere.

Spectrum of Comet I., 1871.

On April 7 a faint comet was discovered by Dr. Winnecke. I observed the comet on April 13 and May 2. On both days the comet was exceedingly faint, and on May 2 it was rendered more difficult to observe by the light of the moon and a faint haze in the atmosphere. It presented the appearance of a small faint coma, with an extension in the direction from the sun.

When observed in the spectroscope, I could detect the light of the coma to consist almost entirely of three bright bands.

A fair measure was obtained of the centre of the middle band, which was the brightest; it gives for this band a wave-length of about 510 millionths of a millimetre. I was not able to do more than estimate roughly the position of the less refrangible band. The result

gives 545 millionths. The third band was situated at about the same distance from the middle band on the more refrangible side.

It would appear that this comet is similar in constitution to the comets which I examined in 1868*.

June 15.—General Sir Edward Sabine, K.C.B., Pres., in the Chair.

The following communication was read :—

"On a Law in Chemical Dynamics." By John Hall Gladstone, Ph.D., F.R.S., and Alfred Tribe, F.C.S.

It is well known that one metal has the power of decomposing the salts of certain other metals, and that the chemical change will proceed until the more powerful metal has entirely taken the place of the other. The authors have investigated what takes place during the process.

The experiments were generally performed as follows :—72 cubic centimetres of an aqueous solution of the salt of known strength, and at 12° Centigrade, were placed in a tall glass ; a perfectly clean plate of metal of 3230 square millimetres was weighed and placed vertically in this solution without reaching either to the top or bottom ; the action was allowed to proceed quietly for ten minutes, when the plate was removed, and the deposited metal was washed off. The loss of weight gave the amount of metal dissolved, and represented the chemical action.

The most complete series of results was with copper and nitrate of silver.

Nitrate-of-silver solution.		Copper dissolved.		Theoretical.	Difference.
Proportional number.	Percentage of salt.	Actual weights.	Average.		
1.	0·3541	0·0045, 0·0050	0·00475	0·00455	+0·0002
2.	0·7083	0·0135, 0·0140	0·01375	0·01365	+0·0001
3.	1·0623	0·0240, 0·0250	0·0245	0·0259	−0·0014
4.	1·4166	0·0420	0·0420	0·0409	+0·0011
5.	1·7705	0·0600	0·0600	0·0583	+0·0017
6.	2·1246	0·0785	0·0785	0·0790	−0·0005
7.	2·4788	0·0975	0·0975	0·0994	−0·0019
8.	2·8332	0·1230, 0·1230	0·1230	0·1228	+0·0002
9.	3·1873	0·1510, 0·1480	0·1495	0·1481	+0·0014
10.	3·5415	0·1680, 0·1670	0·1675	0·1749	−0·0074
11.	3·8956	0·1955	0·1955	0·2035	−0·0080
12.	4·2497	0·2170, 0·2285, 0·2310, 0·2200	0·2241	0·2336	−0·0095
14.	4·9580	0·2740	0·2740	0·2982	−0·0242
16.	5·6664	0·3270	0·3270		
20.	7·0830	0·4510, 0·4400	0·4320		
24.	8·4994	0·5400	0·5400		
30.	10·624	0·6850	0·6850		
32.	11·333	0·7100	0·7100		
40.	14·166	0·8440, 0·9090	0·8765		
48.	16·999	1·0690	1·0690		
60.	21·246	1·359	1·359		
70.	24·788	1·580	1·580		

* Phil. Trans. 1863, p. 555; and Proc. Roy. Soc. vol. xvi. p. 386.

In the earlier terms of this series, *twice the percentage of silver-salt gives three times the chemical action.* The mathematical expression of this law is $c = C \, p^{\frac{\log 3}{\log 2}}$, c being the chemical action, C the constant, and p the proportionate quantity of salt. The close agreement of the observed numbers with those calculated on this supposition as far as the 9th term is shown in the 5th and 6th columns. The law then breaks down, and after about 7 per cent. the increased action is almost in direct ratio with the increased strength.

The position of the plate in the solution was found to make no difference to this 2 : 3 law.

Similar series of experiments were made with zinc and chloride of copper, zinc and sulphate of copper, zinc and nitrate of lead, iron and sulphate of copper, and other combinations; and in every instance where the solution was weak and the action simple, the law of three times the chemical change for twice the strength was found to hold good.

It was proved that the breaking down of the law at about 3·5 per cent. of salt in solution was irrespective of the quantity of the liquid, or of the time for which the plate was exposed. With 72 cub. centims. of a 1·41-per-cent. solution of nitrate of silver the rate of action remained sensibly the same for as long as twenty-five minutes, notwithstanding the constant deposition of silver. This apparently paradoxical result is due to fresh relays of the original solution being brought up to the plate by the currents produced, and that period of time elapsing before any of the products of decomposition are brought back again in their circuit.

When it was perceived that within easily ascertainable limits the chemical action is the same for similar consecutive periods of time, experiments were made in far weaker solutions. It was only necessary to lengthen the time of exposure. It was thus found that the law of three times the chemical action for twice the strength of solution holds good through at least eleven terms of the powers of 2 ; in fact, from a solution that could dissolve one gramme of copper during the hour, to a solution that dissolved only 0·000001 gramme, a million times less.

The manner in which the silver is deposited on a copper plate was examined, and the currents produced were studied. At first a light-blue current is perceived flowing upwards from the surface of the plate ; presently a deep-blue current pours downwards ; and these two currents in opposite directions continue to form simultaneously. A similar phenomenon was observed in every case where a metallic salt attacked a plate of another metal. The downward current was found to be a solution of almost pure nitrate of copper, containing about three times as much NO_3 as the original silver solution, while the upward current was a diluted solution of the mixed nitrates. Moreover the heavy current took its rise in the entangled mass of crystals right against the plate, while the light current flowed from the tops of the crystalline branches. It was evident that when the

fresh silver was deposited on these branches, and the fresh copper taken up from the plate, there was not merely a transference of the nitric element from one combination to another, but an actual mole - cular movement of it towards the copper plate, producing an accu- mulation of nitrate of copper there, and a corresponding loss of salt in the liquid that was drawn within the influence of the branching crystals.   Hence the opposite currents.

The amount of action in a circuit of two metals and a saline so- lution must have as one of its regulating conditions the conducting- power of that solution.   It appeared by experiment that a strong solution of nitrate of silver offers less resistance than a weak one ; and it was also found, on adding nitrate of potassium to the nitrate of silver, that its power of attacking the copper plate was increased, that the augmentation of the foreign salt increased the action still further, and that the 2 : 3 law holds good between two solutions in which both the silver and potassium salt are doubled, though it does not hold good if the quantity of foreign salt be kept constant.   Si- milar results were obtained with mixed nitrates of silver and copper.

While these later experiments offer an explanation of the fact that a solution of double the strength produces more than double the chemical action, they do not explain why it should produce exactly three times the effect, or why the ratio should be the same in all substitutions of this nature hitherto tried.   The simplicity and wide range of the 2 : 3 law seem to indicate that it is a very primary one in chemical dynamics.   _____

GEOLOGICAL SOCIETY.

March 8, 1871.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

The following communication was read :—

" On the Red Rocks of England of older date than the Trias." By Prof. A. C. Ramsay, LL.D., F.R.S., V.P.G.S.

The author stated that the red colour of the Triassic beds is due to peroxide of iron, which incrusts the sedimentary grains as a thin pellicle. This could not have been deposited in an open sea, but rather in an inland salt lake or lakes. The peroxide of iron, which stains the Permian, Old Red Sandstone and Cambrian rocks, is be- lieved by the author to have been deposited in the same manner, in inland waters, salt or fresh.

Agreeing with Mr. Godwin-Austen, the Old Red Sandstone was of Lacustrine origin. The absence of marine shells helps to this con- clusion. The fish do not contradict it ; for some of their nearest living congeners live in African and American rivers.

The life of the Upper Silurian deposits of Wales and the adjoining districts continued in full force up to the passage-beds which mark the change from Silurian to Old Red Sandstone. In these transition strata, genera, species, and individuals are often few, and dwarfed in form. Near Ludlow and May Hill the uppermost Silurian strata contain seeds and fragments of land-plants, indicating the neighbour- hood of land, and the poverty of numbers and the small size of the

shells a change in the condition of the waters. The fish of the Old Red Sandstone also indicate a change of condition of a geographical kind.

The circumstances which marked the passage of Silurian into Old Red Sandstone were as follows :—First, shallowing of the sea, so that the area changed into fresh and brackish lagoons, afterwards converted into great freshwater lakes. At the present day marine species are occasionally found living in fresh water, as for example in the Swedish lakes. The same may have been partly the case in the Old Red Sandstone period. The Old Red Sandstone waters at their beginning are comparable to the Black Sea, now steadily freshening— or the Caspian, once united to the North Sea, if by a change of amount of rainfall and evaporation it freshened by degrees, and finally became a freshwater lake.

The *Permian strata,* to a great extent, consist of red sandstones and marls in the greater part of England ; and the Magnesian Limestone of the north of England is also in less degree associated with red marls. These do not occur in the same districts of England, excepting in Lancashire, where a few beds of Magnesian Limestone are interstratified with the marls. The sandstones and marls being red, the colouring-matter is considered to be due to peroxide of iron, possibly precipitated from carbonate of iron, introduced in solution into the waters.

Land-plants are found in some of the Permian beds, showing the neighbourhood of land. No mollusca are found in most of the red beds, except a brachiopod in Warwickshire, and a few other genera in Lancashire, in marls associated with thin bands of Magnesian Limestone.

The traces of amphibians are like those found in the Keuper Sandstone, viz. *Dasyceps Bucklandi,* and Labyrinthodont footprints in the Vale of Eden and at Corncockle Moor, printed on damp surfaces, dried in the sun, and afterwards flooded in a way common in salt lakes. Pseudomorphous crystals of salt and gypsum help to this conclusion.

The molluscous fauna of Lancashire, small in number, in this respect resembles the fauna of the Caspian Sea. The fauna of the Magnesian Limestone of the east of England is more numerous, comprising thirty-five genera and seventy-six species, but wonderfully restricted when compared with the Carboniferous fauna. The specimens are generally dwarfed in aspect, and in their poverty may be compared to the Caspian fauna of the present day. Some of the fish of the Marl-Slate have strong affinities to carboniferous genera, which may be supposed to have lived in shallow lagoons, bordered by peaty flats ; and the reptiles lately described by Messrs. Howse and Hancock have terrestrial affinities.

Besides the poorness of the Mollusca, the Magnesian Limestone seems to afford other hints that it was deposited in an inland salt lake subject to evaporation. Gypsum is common in the interstratified marls. In the open sea limestone is only formed by organic agency ; for lime, in solution, only exists in small quantities in such a bulk of water ; but in inland salt lakes carbonates of lime and

magnesia might have been deposited simultaneously by concentration of solutions, due to evaporation. Some of the Magnesian-Limestone strata have almost a tufaceous or stalagmitic aspect, as if deposited from solution.

The Cambrian strata also show some evidence of not being true marine deposits. They are purple or red, like the other strata previously spoken of; and the surfaces of the beds sometimes exhibit sun-cracks and rain-pittings. The trilobite *Palæopyge Ramsayi* is considered by the author to be an accidental marking, simulating the form of a trilobite; and the fossils of St. David's are found in grey beds, which may mark occasional influxes of the sea, due to oscillations of level.

The foregoing reasonings, in the author's opinion, lead to the conclusion that in the northern hemisphere a continental area existed, more or less, from the close of the Silurian to the end of the Triassic epoch, and that this geographical continuity of land implies probable continuity of continental genera.

There is therefore no palæontological reason why the *Hyperodapedon, Telerpeton,* and *Stagonolepis* of the Elgin country should be considered of Triassic age, especially as the beds in which they occur are stratigraphically inseparable from the Old Red Sandstone.

Finally, terrestrial and marine European epochs were rapidly reviewed.

1. The Cambrian epoch was probably freshwater.

2. The Old Red Sandstone, Carboniferous, Permian, and Trias were formed during one long continental epoch.

This was brought to an end by partial submergence during the Jurassic epoch; and by degrees a new continental area arose, drained by the great continental rivers of the Purbeck and Wealden series, as shown in various parts of Europe.

3. This continent was almost entirely swallowed up in the Upper Cretaceous seas.

4. By subsequent elevation the Eocene lands were formed; and with this continent there came in a new terrestrial fauna. Most of the northern half of Europe since then has been continental, and its terrestrial fauna essentially of modern type.

If, according to ordinary methods we were to classify the old terrestrial faunas of North America, Europe, Asia, and probably of Africa, a palæozoic epoch would extend from Old Red Sandstone to Wealden times, and a Neozoic epoch at least from the Eocene period to the present day. The Upper Cretaceous strata would at present remain unclassified. The marine epoch would also temporarily be divided into two, Palæozoic from Laurentian to the close of the Permian times; and all besides, down to the present day, would form a Neozoic series. The generic gaps between the two begin already to be filled up. The terrestrial and the marine series at their edges at present overlap each other.

The great life-gaps between the two terrestrial periods may some day be filled up by the discovery of the traces of old continents containing intermediate developments of structure as yet undiscovered.

March 22.—Prof. John Morris, Vice-President, in the Chair.

The following communications were read:—

1. " On the ' Passage-beds ' in the neighbourhood of Woolhope, Herefordshire, and on the discovery of a new species of *Eurypterus,* and some new Land-plants in them." By the Rev. P. B. Brodie, M.A., F.G.S.

The author described as the " passage-beds " between the Silurian and Old Red Sandstone formations near Woolhope, a series of shales and sandstones, which at Perton attain a thickness of about 17 feet. Here the section includes, in descending order :—1. Thin-bedded sandstones; 2. Dark brownish shales; 3. Yellow Sandstone; 4. Olive shales; 5. Thin-bedded sandstone; 6. Olive shales, similar to no. 4. At some localities vegetable remains (*Lycopodites,* and perhaps *Psilophyton*) occur in the olive shales, which also contain several Crustacean fossils, including *Pterygotus Banksii* and a new species of *Eurypterus,* named by Mr. Woodward *E. Brodiei.* Upon this species Mr. Woodward presented a note supplementary to Mr. Brodie's paper.

2. " On the Cliff-sections of the Tertiary Beds west of Dieppe in Normandy and at Newhaven in Sussex." By William Whitaker, Esq., B.A., F.G.S.

The author gave details of the sections of the Tertiary beds at the above places, and noticed the occurrence of London clay. Below this formation, at Dieppe, is a mass of sand, the same as that of the " Oldhaven beds " in East Kent, but here less markedly divided from the clay above; and beneath this sand come the estuarine shelly clays, &c. of the Woolwich beds.

In the older accounts of the Newhaven section a much less thickness of the Tertiary beds is chronicled than may now be seen; indeed the successive descriptions end upwards with higher and higher beds, owing to the destruction of the coast and the wearing-back of the cliff into higher ground, the highest point seeming to have been at last reached.

Here the Oldhaven sand is absent, but the Woolwich clays are in greater force; and the ditch of the new fort shows some very irregular masses of gravel, more or less wedged into those clays.

Both sections show the comparatively wide extent of like conditions to those of the Woolwich beds of West Kent.

3. " On New Tree Ferns and other Fossils from the Devonian." By Prof. J. W. Dawson, LL.D., F.R.S., F.G.S.

The author referred to the numerous species of ferns known in the Upper and Middle Devonian of America, and to the fact that he had described several large petioles as probably belonging to arborescent species, and also two trunks covered with aërial roots, viz. *Psaronius erianus* and *P. textilis.* He also referred to *Caulopteris Peachii* of Salter as the only tree-fern known in the Devonian of Europe.

He then described remains of four species of tree-ferns in collections communicated to him by Dr. Newberry of New York. The first of these, *Caulopteris Lockwoodi,* was found by the Rev. Mr. Lockwood at Gilboa, the locality of the Psaronites already mentioned,

in rocks of the Chemung group. It is a fragment of a well-characterized stem, with parts of five petioles attached to it, and associated with remains of the leaves. It must have been entombed in an erect position, and is not improbably the upper part of one of the species of *Psaronius* from the same locality.

The second species, *Caulopteris antiqua*, Newberry, is of much larger size, but less perfectly preserved. It is a flattened stem on a slab of marine limestone from the Corniferous formation in the lower part of the Middle Devonian (Erian) of Ohio.

The third species, *Protopteris peregrina*, Newberry, is from the same formation with the last, and constitutes the first instance of the occurrence of the genus to which it belongs, below the Carboniferous. The specimens show the form and arrangement of the leaf-scars, the microscopic structure of the petioles, and also the arrangement of the aërial roots covering the lower part of the stem.

The fourth species is a gigantic *Rhachiopteris*, or leaf-stalk, evidently belonging to a species quite distinct from either of the above, and showing its minute structure. It is no less than four inches wide at the base. In the cellular tissue of this petiole are rounded grains similar to those regarded by Corda and Carruthers, in Carboniferous and Eocene specimens, as starch-granules.

In addition to these species, the paper described a new *Nœggerathia* (*N. gilboensis*), and noticed a remarkable specimen from Caithness, in the collection of Prof. Wyville Thomson, throwing light on the problematical *Lycopodites Vanuxemii* of America; also interesting specimens of *Psilophyton* and other genera seen by the writer in the collection of Mr. Peach of Edinburgh.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON THE VELOCITY OF PROPAGATION OF ELECTRODYNAMIC EFFECTS.   BY DR. HELMHOLTZ.

MANY investigators have recently been occupied with the question, how are electrodynamic effects produced at a distance,—whether (according to W. Weber) by forces of the moved electric particles themselves operating immediately on the distant point, but which depend on the velocities and accelerations of these particles in the direction of the line joining them, or (according to C. Neumann, jun.) by forces which diffuse themselves through space with a finite velocity—or whether (according to Faraday and Maxwell) they are occasioned only mediately, by a variation in the medium which fills space. It is indeed a question of prime importance for the foundations of physical science. According to the two last-mentioned views the distant electrodynamic effects of electric currents will not be produced instantaneously, but the impulse to them will be propagated through space with a finite velocity. In the theories of Neumann and Maxwell this velocity is supposed, from electrodynamic measurements, to be nearly equal to that of light. Nevertheless the discussion lately published by me*, of electrodynamic theories, showed that, according to what is accepted respecting the capability

* *Journal für reine und angew. Mathematik*, vol. lxxii. Berlin.

of the air for magnetic and dielectric polarization, other values of this velocity of propagation will agree with the rest of the facts.

Meanwhile a long series of experiments have now been published by P. Blaserna*, from which he concludes that the propagation, at least of the inducing effects of electrical currents, in the air proceeds with a very moderate velocity. According to his experiments with induction-discharges from open circuits (which he considers the most reliable), this velocity in air amounts to only 550 metres, in gum-lac to not more than 330 metres, and is consequently, in the latter case, about equal to the velocity of sound in air. From his experiments with a closed induction circuit he deduced much smaller velocities; yet he has himself acknowledged that the reaction of the induced upon the inducing spiral made the interpretation of the result of his experiments doubtful.

It is to be remarked that in these experiments the distances between the inducing and the induced spiral were very small, varying between 1 and 3 centims.; besides, the two spirals were wound flat. The time corresponding to the propagation through the interval of 2 centims. amounted, in the experiments with open induction, to only $\frac{1}{22000}$ of a second. Neglecting the irregularities occasioned in delicate measurements of time, by the contact between solid metals being always broken by successive leaps, it appeared to me doubtful whether such minute differences of time might not be conditional on the variable duration of the spark at the place of interruption of the inducing current. In my experiments hereinafter described, I convinced myself that, even in much more unfavourable conditions than were present in M. Blaserna's experiments, the interruption-spark may have a duration of $\frac{1}{46000}$ of a second; MM. Lucas and Cazin recently found $\frac{1}{80000}$ for larger electrical batteries with 2·292 millims. striking-distance, $\frac{1}{15000}$ with 5 millims. striking-distance†. While M. Blaserna employed several Bunsen's elements for the production of the current, I used only one Daniell's element; and while his spirals contained close-pressed coils of wire, mine had a very large periphery and few coils, and was therefore much less calculated to produce a strong extra-current; and yet the duration of the spark reached the quantity stated. Now, when we consider that, as is well known, the approach of a second spiral, in which an induction-current is produced, seriously diminishes the intensity of the spark, because the induced current counteracts the inducing, and that M. Blaserna's spirals were always proportionally very near each other, the doubt occurs whether the longer duration of the spark did not produce an apparent retardation of the operation with a greater distance of the spirals.

As I had for a long time been occupied with experiments on the course of electric currents of very short duration, and had had apparatus manufactured for this purpose, it seemed to me before all things necessary to prove whether the velocity of propagation of electrodynamic effects has really so low a value as M. Blaserna has concluded.

* *Giornale di Scienze Naturale ed Economiche*, vol. vi. 1870. Palermo.

† M. J. Bernstein, in a close-coiled spiral of fine wire, $\frac{1}{20000}$ (Poggendorff's *Annalen*, vol. cxlii. p. 65).

The experiments I have hitherto carried out refer to the propagation through air only. The very note-worthy influence of electric insulators shown in the Italian physicist's experiments required still further study. That also in insulators electrical movements of very brief duration occur which in some circumstances may well operate inductively on their vicinity, similarly to the excitement of magnetism in iron, appears very probable from the influence which such media have as dielectrics. For the time I did not prosecute this part of the inquiry. The interruption-apparatus used by me for the conduction of the currents consisted of a heavy and solid iron pendulum, the support of which was let into the wall, and which was always let fall from the same height. At the lower end it had two projections overlaid with plates of agate, which at the moment when the pendulum passed through the position of equilibrium struck the steel ends of two light little levers, by the motion of which two current-conductions were interrupted. One of these levers rested on a fixed support, the other on a slide which could be shifted by means of a micrometer-screw, so that the stroke on this moveable lever resulted, by any small period chosen, now sooner, now later than that on the other. The interval of time was calculated from the micrometrically measured displacement of the striking-point and from the velocity of the fall of the pendulum : the latter was calculated from the time and arc of oscillation of the pendulum. A division-mark on the head of the micrometer-screw corresponded to $\frac{1}{231170}$ of a second. With present arrangements it would have been useless to take more accurate readings, on account of the inequality of duration of the spark.

As it was important to have the distances between the spirals as great as possible, I gave to them the form of rings of about 80 centims. diameter. The inducing spiral had only $12\frac{1}{4}$ turns of copper wire 1 millim. thick, covered with $\frac{1}{2}$ millim. thickness of gutta percha. The induced spiral, on the contrary, had 560 turns of copper wire, spun round with silk, of $\frac{1}{2}$ millim. diameter. This spiral could be placed at a distance of 170 centims. from the inducing one without the inducing effect ceasing to be evident. But even the nearest distance to which the two coils were brought amounted still to 34 centims., in order to make the reaction of the induced on the inducing current imperceptibly small—which, so far as could be judged by time-measuring experiments, was accomplished.

In the experiments the following was the arrangement adopted :—

The circuit of the inducing current contained a Daniell's element, the smaller spiral, and the first-struck place of interruption. By the stroke the current was stopped ; and its interruption operated inducingly on the alone remaining second circuit. This was not perfectly closed, but its ends led to a condenser (after Kohlrausch) with two gilt metal disks, which were brought to $\frac{3}{8}$ millim. distance from one another. This circuit consisted of the larger wire spiral, one end of which was connected immediately with the fixed plate of the condenser, in communication with the earth. The other end communicated, through the second interruption-lever, with the insulated moveable plate of the condenser. The electricity put in motion by induction thus flowed into the condenser up to the moment when

the second interruption-place was struck. Thenceforth the move-able plate of the condenser was insulated, and retained the charge it had received.

Its quantity and kind were then measured by an electrometer con-structed on Sir W. Thomson's principle, after removing the plates of the condenser one from the other.

The process which was thus submitted to observation is therefore the series of electrical oscillations remaining, after the interruption of the primary current, in the induced spiral connected with the condenser. Since these oscillations proceed from one plate of the condenser to the other in an unbroken conduction without any issue of sparks, they pass much more regularly and are much more nume-rous than those observed in the connecting arcs of Leyden batteries. The length of my micrometer permitted the reading off of 35 posi-tive and the same number of negative phases when the distance between the spirals was 34 centims. The time of an entire oscilla-tion (positive and negative together) amounted to $\frac{1}{2511}$ of a second; the total duration of the 35 oscillations observed was therefore $\frac{1}{80}$ of a second; and when I was obliged to break off the observations, the oscillations were still by no means so feeble that, with a greater play of the micrometer, a long series of them could not have been observed.

The result of experiments in which I varied the distance of the condenser-plates, and consequently the electric capacity of the con-denser, was, that the duration of the oscillations was but very little affected by the capacity of the condenser. I have, in a previous communication*, pointed out that even a close-wound spiral itself operates as a condenser, since the turns at one end become charged positively, and those at the other end negatively, and are only sepa-rated from the less strongly charged positive or negative layers in their vicinity by the very thin insulating layers of silk; hence an electric coating is accumulated on both sides of the insulating layer. Since the lessening of the capacity of the condenser has so little in-fluence on the duration of the oscillation, it follows that the conden-satory capacity of the spiral must have considerably exceeded that of the condenser.

In order to discover the possible retardation of the distant effect, it was necessary to discover a conspicuous point in the course of the electric oscillations that could be very sharply determined. The first moment of the commencement was unsuitable, since, apparently, the current commences with a velocity rising from zero, and this only gradually increases; hence the strength of the current at first in-creases, at the most, proportionally to the square of the time. The reason of this is to be sought in the fact that the primary current also, during the time of the spark, vanishes only gradually, and hence the induced electromotive force in the secondary circuit is by no means suddenly developed there in perfection, but itself first

* *Verhandlungen des naturh. med. Vereins zu Heidelberg*, April 30, 1869. In the earlier observations, with the aid of the current-testing leg of a frog, I had verified as many as 45 oscillations, the total duration of which amounted to $\frac{1}{60}$ of a second.

grows continuously. But in our case the duration of the spark is about equal to the tenth part of the whole period of an oscillation, and therefore by no means inconsiderable *. Its average length is determined by comparing the time between the stroke of the pendulum which breaks the primary conduction and the first zero-point of the current, with the time between the successive zero-points. The former is longer, because, besides the period of half an oscillation, it comprises also the duration of the spark; and the amount of this can be approximately found by means of such a comparison.

On the other hand, the successive zero-points of the current can be very sharply determined, even with the more distant position of the secondary spiral. As, even with such feeble electromotive forces and spirals of so few turns as I used in the primary circuit, the duration of the spark is never constant (the reason of which is probably to be sought in the throwing-off of platinum particles by the sparks), the deflections about the division-mark corresponding to zero are, even with a good arrangement of the apparatus, sometimes positive, sometimes negative; on the contrary, at the preceding and succeeding divisions they are either exclusively or preponderantly in one definite direction.

With the arrangement described, at which I had arrived after many trials, it was shown that *the greater distance of the two spirals* (136 *centims.*) *did not alter the situation of the zero-point of the induced current by one division of the micrometer—that is to say, by* $\frac{1}{231170}$ *of a second. If, then, the inducing effects are actually propagated with a definable velocity, this must be greater than* 314,400 *metres, or about* 42·4 *geographical miles* [about 195 British statute miles] *in a second.*

I have hit upon preparations for a further refinement of these measurements. How far this can be carried, will, it appears to me, depend chiefly on how far the spark at the place of interruption can be reduced when a very small resistance and very small electromotive force are given to the primary circuit, and when the turns of wire forming it are removed as far as possible from one another.—*Monatsber. d. Kön. Preuss. Akad. Berlin,* May 1871, pp. 292–298.

ON A NATIVE SULPHIDE OF ANTIMONY FROM NEW ZEALAND. BY
M. M. PATTISON MUIR, F.C.S., STUDENT IN THE LABORATORY
OF THE ANDERSONIAN UNIVERSITY, GLASGOW†.

In the gold mines at the Thames (New Zealand) there are found tolerably large quantities of grey antimony ore or stibnite, associated with the quartz and other rocks of the older series from which gold is extracted. The analysis of a sample of this stibnite, which I obtained about a year ago, I have now the honour to lay before the Association.

* The relatively slow decrease of intensity of the primary current during the spark is also evidently the reason that, as I previously found, in an induced spiral with more than 7000 electric oscillations in a second the latter turned out so feeble : the interruption is then not sufficiently sudden for the brief duration of the oscillation.

† Communicated by Prof. T. E. Thorpe, having been read at the Meeting of the British Association held at Edinburgh, August 1871.

The sample had the appearance of a large mass of steel-grey crystals, radiating chiefly from a central point; some of the crystals being fully an inch in length, and generally very perfectly formed. The crystals were prisms belonging to the trimetric system, soft, and easily cut with the knife, in the direction parallel to the principal axis, showing when cut a brilliant metallic lustre.

Adhering to the crystals was a small amount of gangue, composed seemingly of siliceous matter.

For the purpose of analysis, a large crystal was broken off perfectly free from any foreign matter. The specific gravity of the crystals $= 4·625$. The following are the results of the analysis :—

Antimony	71·09
Iron	0·24
Arsenic	traces
Sulphur	28·47
	99·80

ON THE REVERSAL OF THE LINES OF THE SPECTRA OF METALLIC VAPOURS. BY M. A. CORNU.

In studying the spectrum of the spark of magnesium, which serves me as a monochromatic light for photographing the coloured rings[*], I have been led to a series of observations which appear to have some interest in respect of the study of the solar spectrum.

My purpose was to photograph the line, in the spectrum of the magnesium-spark, which produces by itself alone the greater portion of the photogenic energy. This line is triple[†]; it is situated beyond the violet, between H and L, but very near the latter; it is the repetition (so to say, the sharp harmonic) of the line b of the visible spectrum, the wave-lengths being nearly in the ratio of 3 to 4.

The experiment had succeeded a great number of times; nevertheless I was desirous of repeating a series of measurements of refrangibility and wave-lengths with a more powerful pile and a very effective induction-apparatus[‡].

To my great surprise, the spark, really prodigious, produced scarcely any photographic impression, relatively to what I had expected to obtain; it was also necessary to prolong the time of exposure to two minutes, it having been estimated, from the strength of the spark, at two or three seconds. The examination of the plate revealed an unexpected phenomenon : instead of the usual three lines, there were five; the two least refrangible were distinctly doubled; yet the lines were very broad, and their exterior contours ill defined. My first idea was to suppose an error in fixing the position of the apparatus (*mise au point*); but on substituting iron elec-

[*] *Comptes Rendus*, 1869, vol. lxix. p. 333 : "Méthode optique pour l'étude de la déformation de la surface extérieure des solides élastiques."

[†] *Comptes Rendus*, 1869, vol. lxix. p. 337 : "Sur les spectres ultra-violets," by M. Mascart.

[§] A large induction-coil, fed by a pile of 16 Bunsen couples arranged as 8 elements with double surface; condenser spherical, of 40 centims. diameter; electrodes of magnesium, consisting of two pieces of metal of 10 millims. thickness.

trodes for the magnesium ones, the lines of the iron-spectrum (espe-
cially the one situated between the two doubled magnesium-lines)
photographed themselves with perfect distinctness , there was there-
fore no error on this side.

I then concluded that the doubling of the lines was a true *reversal*—
the analogue, in the invisible portion of the spectrum, of the reversal
of the line D, obtained by M. Fizeau on placing between the two
carbon points of the electric lamp a fragment of sodium.

No hesitation would have been possible if the three lines had been
doubled simultaneously ; but it seemed strange that one of them
should escape this modification. I successively took twenty-two
plates ; and in none did the most refrangible line appear doubled.

I then recollected an observation of Father Secchi on the spec-
trum-analysis of a solar spot (May 1869)*, in which the able astro-
nomer relates his having witnessed the reversal of one only (the
least refrangible) of the three magnesium-lines which constitute *b*.
This near resemblance induced me to persevere ; and I was fortu-
nate enough to produce at will, on the same plate, the reversed or
the normal lines.   One can even compare the positions of the normal
and the reversed lines, just as the spectrum of an artificial light is
compared with that of the sun.   It is only necessary to cause the
spark of a powerful induction-apparatus to issue between the mag-
nesium electrodes, covering one half of the slit of the spectroscope,
then the spark from a very feeble apparatus †, prolonged a sufficient
time, and to cover the other half of the slit.   The exact coincidence of
the normal and reversed lines is easily ascertained.

The reversal in the ultra-violet portion being beyond doubt, it was
necessary to obtain it also in the visible region of the spectrum.
The spark of the large induction-coil, even with 24 couples arranged
as 12 with a double surface, produced no effect on the magnesium-
line *b* ; but the experiment succeeded easily with the voltaic arc of a
pile of 50 couples.   It can be effected thus :—For the positive pole a
disk of carbon 6–8 centims. in diameter is taken, in which are sunk
little capsules ; in each of these is placed a fragment of metal ; then
the negative carbon is lowered, and then raised as soon as the spark
has issued, so as to obtain an arc of 4 or 5 millims.   With the aid of
a lens the image of the arc is projected on the slit of the spectroscope.
In the case of magnesium the triple line *b* appears bright and clean :
it is to be *placed exactly in position*.   Then the upper carbon is
lowered by degrees ; the lines widen, become blurred, and very soon
a very fine black line appears on the least refrangible ; if the ap-
proximation of the carbons be continued, the second and at last the
third line will be reversed.   Here, then, is the verification of the
phenomenon observed photographically, and the artificial reproduc-
tion, although in the opposite direction, of that described by Father
Secchi.

* *Comptes Rendus*, vol. lxviii. p. 1243.   The author adds that the line
C "appeared sometimes doubled ; but," he says, " I attribute this pecu-
liarity to the movement of the terrestrial atmosphere."   May not this also
have been a true reversal ?
† A medium induction-coil, 3 or 4 couples, and a single Leyden jar of
about a litre capacity.

It is remarkable that all the lines of the spectrum are not reversed simultaneously. Thus the very bright violet line situated nearly in the centre of the space between the two triple lines shows no trace of reversal. In an early communication I will describe another series of experiments, which tend to class the lines of one spectrum in different categories. It will be sufficient for me here to say that the only lines I have succeeded in reversing belong to the light emitted by the atmosphere of vapour which envelopes the incandescent metal*.

After this first experiment, I tried the various metals I had at hand. The same phenomenon was reproduced in divers groups of lines; and in general the reversal commences with the least refrangible line of the group, and only continues if the temperature is progressively raised.

The following are, for the principal metals, the groups of lines which I have been able to reverse. The metals are arranged nearly in the order of the facility of production of the phenomenon; the lines are designated by their wave-lengths expressed in millionths of a millimetre.

Sodium	.... line D	$\lambda = 589$	
Thallium	.... green line	$\lambda = 535$	
Lead	.... violet line...	$\lambda = 406$	
Silver	{ green lines	$\begin{cases} \lambda = 546 \\ \lambda = 521 \end{cases}$	
	violet line...	$\lambda = 424$†	
Aluminium.	{ violet lines between	$\begin{cases} \lambda = 396 \\ \lambda = 394 \end{cases}$	
	H_1 and H_2		
Magnesium.	{ triple green line ..	$\lambda = 518 \cdot 3$, the least refrangible.	
	triple line beyond the violet, near L.	$\lambda = 383 \cdot 78$‡, the least refrangible.	
Cadmium ..	{ green line	$\lambda = 509$	
	bluish-green line ..	$\lambda = 480$	
	blue line	$\lambda = 467 \cdot 7$	
Zinc	{ green line	$\lambda = 461$	
	green line	$\lambda = 472$	
	blue line	$\lambda = 467 \cdot 8$	
Copper	...... green line	$\lambda = 510$	

Iron, cobalt, bismuth, antimony, and gold have given no appearance of reversal. The alkaline salts, especially the chlorides, still more easily produce the reversal of the bright lines. The chlorides of sodium ($\lambda = 589$ and 422) and lithium ($\lambda = 670$ and 458) are the most remarkable.

* It is understood that the bright lines due to air and aqueous vapour must not be confounded with those of the metal, which furnishes at least two sorts of bright lines.

† Perhaps really double.

‡ Determined photographically, with a Nobert's network. The deviation was $11° 46' 40''$ (spectrum of the 2nd order). The analogue of group b ($\lambda = 518 \cdot 3$, according to M. Ångström) was deviated $16° 0' 5''$. The ratio between the wave-lengths is $0 \cdot 74046$. The mean wave-length of the group is equal to $383 \cdot 3$.

These experiments confirm the theory of the reversal of the lines by absorption ; for the condition of success is the bringing together, as completely as possible, of the carbons, consequently the formation of a large quantity of vapour in a very small space.   In the centre the temperature is very high, the radiations very intense ; the cor- responding lines are at the same time bright and widened ; around the central focus the layers are cooler, they emit radiations less in- tense, but clearer as the length of an undulation.   Thus the reversal line is very fine when the thickness is sufficient to produce absorp- tion ; it would become wider and wider, as happens in the case of soda, if the temperature were still more raised.

I will add a few words in order to set forth again the connexion between these experiments and spectrum-observations of the sun.

When Kirchhoff gave the explanation of the reversal of the lines, his view was admitted, that the dark lines of the solar spectrum were due to a continuous atmosphere enveloping the sun and ab- sorbing certain radiations of the photosphere.   Nevertheless, in the chemical point of view, the constitution of this atmosphere, in which would be found pell-mell the vapours of so many different bodies, presents some difficulties ; besides, its existence is contradicted by the comparative observations of the margin and the centre of the sun.   If the absorbent atmosphere had a sensible thickness, the ab- sorption-spectrum would vary with the thickness traversed by the rays which reach us, and consequently would have a different aspect at the centre and at the margin of the solar disk.   We know that it is not so ; and hence astronomers have concluded that the emis- sion of luminous radiations, and the absorption of certain of them, both take place on the photosphere itself.   This hypothesis is veri- fied by our experiments, and even precisely defined to a certain degree ; for they show :—

1st. That a very slight thickness of vapour can produce the re- versal of the lines—a thickness absolutely imperceptible at our dis- tance from the sun ; and

2ndly. That there is no advantage in supposing a continuous at- mosphere, however thin, the absorption being *all local*, and produced *spontaneously* by the external cooling about each incandescent point.

It will moreover be remarked that the experiment described above is a genuine reproduction of the hypothetical constitution of the sun, and a synthesis of the spectrum-phenomena presented by it,—the incandescent carbon, on which is the metal, playing approximately the part of the photosphere, with a layer of vapours above at a very high temperature and emitting radiations which are partially absorbed by the exterior layer.

In short, a new fact brought out by these experiments is the re- versal of a great number of metallic lines : hitherto success had been obtained in reversing scarcely any besides the sodium-line. Moreover they confirm the observations (which at first appeared very singular) of the partial reversal of a group of lines, by showing that this apparent singularity is the general case.   And, lastly, they justify, and even precisely define, certain hypotheses concerning the constitution of the sun.—*Comptes Rendus*, July 21, 1871.

XXIX. *On Ocean-currents.*—Part III. *On the Physical Cause of Ocean-currents.* By JAMES CROLL, *of the Geological Survey of Scotland.*

[Continued from vol. xl. p. 259.]

Dr. Carpenter's *Theory of a General Oceanic Circulation.*

THE two great causes which have been assigned for ocean-currents are the influence of the winds and the difference of specific gravity between the ocean in equatorial and polar regions. But even amongst those who adopt the former theory, it is generally held that the winds are not the sole cause, but that, to a certain extent at least, difference of specific gravity contributes to produce motion of the waters. This is a very natural conclusion; and in the present state of physical geography on this subject one can hardly be expected to hold any other view. It is only when we adopt the more rigid method of determining in absolute measure the amount of the forces resulting from difference of specific gravity that we become aware that this is a cause utterly insufficient to produce the motions attributed to it. In my last paper* I examined at considerable length Lieut. Maury's theory, and endeavoured to show that difference of specific gravity between the sea in equatorial and polar regions could not, as he supposed, be the cause of the Gulf-stream and other currents. Since the publication of that paper, an interesting and elaborate memoir on ocean-currents has been read before the Royal Geographical Society by Dr. Carpenter. In this memoir he states that my objections do not apply to the slow movement, imperceptible to

* Phil. Mag. for October 1870.

ordinary observation, which he advocates. The conclusion at which I arrived was that the motive force resulting from the difference of specific gravity is equal to one fourth of a grain on the cubic foot (63 lbs.) ; and I also adduced evidence to show that a force so infinitesimal is not only insufficient to move the water at the rate of several miles an hour, as in the case of the Gulf-stream and other currents, but is actually insufficient to produce any sensible motion whatever,—in short, that Dr. Carpenter's theory of a general interchange of Equatorial and Polar water resulting from difference of specific gravity is quite as physically impossible as Lieut. Maury's theory. But as Dr. Carpenter, in the memoir referred to*, has stated his theory with much fulness, and supported his positions with much ingenuity of argument, it will be necessary for me to enter a little more minutely into some of the points under discussion before considering the influence of the winds as a cause of currents. This will, undoubtedly, be best effected by an examination of Dr. Carpenter's arguments. Until these are shown to be insufficient to support the theory maintained by him, it is needless to begin the consideration of the effects of the winds. And as he has in his memoir done me the honour to discuss at considerable length some of my objections to his theory, I trust that it will not be deemed either discourteous or presumptuous in me that I should enter somewhat more fully into the subject.

There are three ways whereby it may be determined whether or not the circulation of the ocean is due to difference in specific gravity : viz. (1) the matter may be determined by direct experiment ; (2) it may be determined by ascertaining the absolute amount of *force* acting on the water to produce motion, in virtue of difference of specific gravity, and then comparing it with the force which has been shown by experiment to be necessary to the production of sensible motion ; or (3) by determining the greatest possible amount of *work* which gravity can perform on the waters in virtue of difference of specific gravity, and then ascertaining if the work of gravity does or does not equal the work of the resistances in the required motion. It is strange that Dr. Carpenter nor any of the advocates of the gravitation theory have ever adopted any of these methods. Dr. Carpenter seems to take for granted that a circulation of the ocean similar to what he advocates is a " physical necessity." But this is the very point at issue. If the work of gravity so far exceeds the work of the resistances in such a motion of the waters as that supposed by Dr. Carpenter, then, indeed, such a motion is a physical necessity ;

* " On the Gibraltar Current, the Gulf-stream, and the General Oceanic Circulation," Proceedings of the Royal Geographical Society, vol. xv. p. 54.

but, on the other hand, if the work of gravity falls short of the work of the resistances (and Dr. Carpenter has nowhere proved that it does not), the motion which he supposes is not only not a physical necessity, but is actually a physical impossibility.

Dr. Carpenter states that his doctrine of a General Oceanic Circulation has been accepted as valid by some of the most distinguished mathematicians and physicists of this country. This is, no doubt, true; but I cannot help thinking that those eminent physicists who have given a general assent to the correctness of his theory have done so without giving it special consideration—and that when they come to examine the question more minutely they will be satisfied that the forces resulting from difference of density, whether that difference be caused by difference of temperature or by difference in saltness, is so infinitesimal as to be wholly insufficient to produce the great currents of the ocean. With the exception of Dr. Colding, of Copenhagen*, I am not aware that any physicist holding the gravitation theory has attempted to determine the absolute amount of the forces acting on the water so as to produce motion.

Dr. Carpenter's experiment.—True, Dr. Carpenter has exhibited an experiment to show the motion of the water. But I presume his experiment was intended rather to illustrate the way in which the circulation of the ocean, according to his theory, takes place, than to prove that it actually does take place. At any rate all that can be claimed for the experiment is the proof that water

* Dr. Colding, of Copenhagen, in a memoir lately published (*Om Stromningsforholdene i almindelige Ledninger og i Havet*, 1870), has determined with much labour and skill the influence of difference of specific gravity and of the earth's rotation as causes of the Gulf-stream. The following are some of the conclusions at which he has arrived. Between Bemini and St. Augustine, the only motive power he considers is difference of level, which he estimates to be 6 feet. From St. Augustine to New York Bay the stream is propelled by the rotation of the earth, the force of which is equal to that of a slope of 9 or 10 feet. From New York Bay to Europe it is propelled east by rotation up a slope of about 1 foot. Near Europe the current divides into two branches. One, under the influence of the diminished force of rotation, goes south-east to the coast of Africa; the other goes along the British coast, and is turned north by the direction of the coast, rotation causing it to rise from left to right about $1\frac{1}{2}$ foot. The estimated force of rotation exercised on the Gulf-stream from St. Augustine to lat. 60° N. he considers to be equal to that of a difference of level of 25 feet. He has in like manner shown the influence of difference of level and rotation on the return current from the Arctic regions to the Gulf of Mexico.

Dr. Colding appears, however, to leave out of account molecular resistance to motion. He says, "we do not know at present if the molecules of water or air move without resistance." But, for reasons which will come under our consideration, Dr. Colding appears to have greatly overestimated the influence of gravity in the production of motion.

R 2

will circulate in consequence of difference of specific gravity re-
sulting from difference of temperature. But this does not require
proof; for no physicist denies it. The point which requires to
be proved is this. Is the difference of specific gravity which
exists in the ocean sufficient to produce the supposed circulation?
Now his mode of experimenting will not prove this, unless he
makes the conditions of his experiment agree with what actually
exists in the ocean. These conditions I have already stated at
considerable length in my last paper*. If his trough be as
much as 1 inch in depth, it will require to be upwards of 120
feet in length. Let the surface-temperature of the water at one
end of such a trough be 80°, decreasing from the surface down-
wards till at the depth of half an inch it is as low as 30° or 32°,
and let the water at the other end be kept at 32°, or as low as
it can be kept without freezing. If the experiment succeeds
under these conditions, his point will be established.

But I most decidedly object to the water being heated in the
way in which it has been done by him in his experiment before
the Royal Geographical Society; for I feel pretty confident that
in this experiment the circulation resulted not from difference of
specific gravity, as was supposed, but rather from the way in
which the heat was applied. In that experiment the one half of
a thick metallic plate was placed in contact with the upper sur-
face of the water at one end of the trough ; the other half, pro-
jecting over the end of the trough, was heated by means of a spirit-
lamp. It is perfectly obvious that though the temperature of
the great mass of the water under the plate might not be raised
over 80° or so, yet the molecules in contact with the metal would
have a very high temperature. These molecules, in consequence
of their expansion, would be unable to sink into the cooler and
denser water underneath, and thus escape the heat which was
being constantly communicated to them from the heated plate.
But escape they must, or their temperature would continue to
rise until they would ultimately burst into vapour. They can-
not ascend, neither can they descend, but will be expelled by the
heat from the plate in a horizontal direction. The next layer of
molecules from beneath would take their place and would be ex-
pelled in a similar manner, and this process would continue so
long as the heat was applied to the plate. A circulation would
thus be established by the direct expansive force of vapour, and
not in any way due to difference of specific gravity, as Dr. Car-
penter supposes.

The case referred to by him of the heating-apparatus in the
London University is also unsatisfactory. The water leaves the
boiler at 120° and returns to it at 80°. The difference of spe-

* Phil. Mag. Oct. 1870, p. 254.

cific gravity between the water leaving the boiler and the water returning to it is supposed to produce the circulation. It seems to me that this difference of specific gravity has nothing whatever to do with the matter. The cause of the circulation must be sought for in the boiler itself, and not in the pipes. The heat is applied to the bottom of the boiler, not to the top. What is the temperature of the molecules in contact with the bottom of the boiler directly over the fire, is a question which must be considered before we can arrive at a just determination of the causes which produce circulation in the pipes of a heating-apparatus such as that to which Dr. Carpenter refers. But, in addition to this, as the heat is applied to the bottom of the boiler and not to the top, convection comes into play, a cause which, as we shall find, does not exist in the theory of oceanic circulation at present under our consideration.

But, be all this as it may, though I do not believe that in Dr. Carpenter's experiments circulation is effected by difference of specific gravity, still I freely admit that difference of specific gravity will produce a circulation such as that supposed by him. Neither do I deny that the thing can be shown experimentally. What I affirm is, that no experiment can show that water will circulate under difference of specific gravity if the conditions of the experiment be made to agree with what exists in the ocean; and unless these conditions are complied with, any experiment, no matter what it may be, is useless so far as concerns the question at issue.

The Force exerted by Gravity.—Sir John Herschel, in proving that difference of specific gravity could not be the cause of ocean-currents, adopted the second of the three methods to which I have referred; viz. he showed that the *force* of gravitation, acting on the waters of the ocean in virtue of specific gravity, is not sufficient to produce the required motion. Sir John in his calculations had taken 39° as the temperature of maximum density. The temperature of maximum density, however, is much lower than this; and as Dr. Carpenter maintained that all determinations based upon the supposition that 39° is the temperature of maximum density gave too low an estimate of the effect of difference of specific gravity in causing motion, I calculated in my paper what would be the force of gravity, taking 32° to be the temperature of the greatest density instead of 39°, and found that the force of gravitation was about the same as when the temperature was taken at 39°,—the reason being, that when we take 32° as the temperature of maximum density, we have 18 feet as the height of the ocean at the equator above the place of maximum density, but then this place is the pole, whereas when we take 39° as the temperature of maximum

density, though we have only 10 feet as the height of the ocean at the equator above the place of maximum density, this place is at lat. 56° instead of at the pole. Now a slope of 10 feet from the equator to lat. 56° is about the same in steepness as a slope of 18 feet from the equator to the pole. It therefore follows that Herschel, in taking 39° as the temperature of maximum density, did not underestimate the effect of gravity.

Dr. Carpenter nowhere, so far as I am aware, calls in question my estimate of the height at which the surface of the ocean at the equator stands above that at the poles. He does not say that the slope from the equator to the poles is greater than I have estimated it to be, nor does he say that I have underestimated the force of gravity impelling the water down this slope, in concluding it to be only one fourth of a grain on one cubic foot (63 lbs.) of water. Neither does he affirm that a force so infinitesimal could produce the necessary circulation. On the contrary, he admits that it would not*, and says that I *justly* maintain that a circulation could not be sustained by this means. He appears to admit my results so far as I have gone, but maintains that I have not gone far enough. I have justly estimated the effects of heat, but I have, he says, entirely ignored the agency of cold. "Mr. Croll," he says, "in arguing against the doctrine of a General Oceanic Circulation sustained by difference of temperature, and justly maintaining that such a circulation cannot be produced by the application of heat at the surface, has entirely ignored this agency of cold "*. And this agency of cold which I have ignored he considers is of far greater importance than the one which I advocate. The agency of cold he regards as the *primum mobile* of the general oceanic circulation.

But surely Dr. Carpenter is mistaken in supposing that I have entirely ignored the agency of cold. In what I have advanced, as much has been attributed to cold as to heat. The height of the surface of the ocean at the equator above the surface at the poles is, in my opinion, as much due to cold as to heat. The slope is due to the *difference* of density between the equatorial and polar waters. Now this difference is just as much the result of the contraction of the polar water by cold as of the expansion of equatorial water by heat.

It is evident that the agency of cold referred to by Dr. Carpenter, which I have ignored, must be something else than the mere influence of cold in the production of the slope by the contraction of the polar waters. The cold, according to him, must exercise some power in the way of producing motion of the water *over and above* what is derived in virtue of the slope; and

* See footnote to § 25.

also this power, whatever may be the way in which it acts, is far more effective in producing motion than the slope; for he regards it as the *primum mobile* of the whole affair, whereas the tendency of the water to run down the slope is regarded as a secondary matter.

It is perfectly true that I have not in my paper on the subject taken into account any such agency as this supposed by Dr. Carpenter, for the simple reason that I know of no such agency. With the exception of an exceedingly trifling force, which I did not deem worthy of being taken into account, and to which I shall have occasion shortly to refer, I know of no possible agency arising from difference of specific gravity tending to produce circulation other than the force impelling the water down the slope.

The Work performed by Gravity.—But in order clearly to understand this point, it will be better to treat the matter according to the third method, and consider not the mere *force* of gravity impelling the waters, but the amount of *work* which gravitation is capable of performing.

Assuming, then, the correctness of my estimate, that the height of the surface of the ocean at the equator above that at the poles is 18 feet, a pound of water in flowing down the slope from the equator to either of the poles will perform 18 foot-pounds of work. Or, more properly speaking, in the descent of a pound of water down this slope from the equator to the pole gravitation performs 18 foot-pounds of work. Now it is evident that when this pound of water has reached the pole, it is at the bottom of the slope, and consequently cannot descend further. Therefore gravity cannot perform any more work upon it; for gravity cannot perform work unless the thing acted upon descend—that is, moves under the force exerted. But the water will not move under the influence of gravity unless it move downward; for it is only in this direction that gravity acts on the water. " But," says Dr. Carpenter, " the effect of surface-cold upon the water of the polar basin will be to reduce the temperature of its whole mass below the freezing-point of fresh water, the surface stratum *sinking* as it is cooled in virtue of its diminished bulk and increased density, and being replaced by water not yet cooled to the same degree." (§ 22.) The cooling of the whole mass of polar water by cold and the heating of the water at the equator by the sun's rays make, of course, as we have just seen, the polar column of water to be denser than the equatorial one, and consequently, in order that the two may balance each other, the polar column is shorter than the equatorial by 18 feet; and the slope of 18 feet to which we allude is thus formed. It is perfectly true that the water which leaves the equator warm

and light, by the time it reaches the pole becomes cold and dense. But unless it be denser than the polar water underneath, it will not sink down *through it**. But why should it be colder than the " whole mass " underneath, which, according to Dr. Carpenter, is cooled by polar cold ? He does not explain how this becomes the case. But that he does suppose it to sink to the bottom in consequence of its contraction by cold would seem from the following quotation :—

" Until it is clearly apprehended that sea-water becomes more and more dense as its temperature is reduced, and that it consequently continues to sink until it freezes, the immense motor power of polar cold cannot be apprehended. But when this has been clearly recognized, it is seen that the application of *cold at the surface* is precisely equivalent as a moving power to that application of *heat at the bottom* by which the circulation of water is sustained in every heating-apparatus that makes use of it." (§ 25.)

Here he says that the application of cold at the surface is equivalent as a motor power to the application of heat at the bottom. But the way in which heat applied to the bottom of a vessel produces circulation is by *convection*. It makes the molecules at the bottom expand, and they, in consequence of buoyancy, rise *through* the water in the vessel. Consequently if the action of cold at the surface in polar regions is equivalent to that of heat, the cold must contract the molecules at the surface and make them sink *through* the mass of polar water beneath. But assuming this to be his meaning, how much colder is this surface-water than the water beneath ? Suppose there is one degree of difference. How much work, then, will gravity perform upon this one pound of water which is one degree colder than the mass beneath supposed to be at 32° ? The force with which the pound of water will sink will not be proportional to its weight, but to the difference of weight between it and a similar bulk of the water through which it sinks. The difference between the weight of a pound of water at 31° and an equal volume of water at 32° is $\frac{1}{29000}$ of a pound. Now this pound of water in sinking to a depth of 10,000 feet, which is about the depth at which a polar temperature is found at the equator, would perform only one third of a foot-pound of work. And supposing it were three degrees colder than the water beneath, it would in sinking perform only one foot-pound. This would give us only

* It is a well-established fact that in polar regions the temperature of the sea decreases from the surface downwards ; and the German Polar Expedition found that the water in very high latitudes is actually less dense at the surface than at considerable depths, thus proving that the surface-water could not sink in consequence of its greater density.

$18 + 1 = 19$ foot-pounds as the total amount that could be per-
formed by gravitation on the pound of water from the time that
it left the equator till it returned to where it started. The
amount of work performed in descending the slope from the
equator to the pole and in sinking to a depth of 10,000 feet or
so through the polar water assumed to be warmer than the sur-
face-water, comprehends the total amount of work that gravita-
tion can possibly perform ; so that the amount of force gained
by such a supposition over and above that derived from the slope
is trifling.

But it would appear that this, after all, is not what Dr. Car-
penter means, but something entirely different. What he means
seems to be this : when a quantity of water, say a layer one foot
thick, flows down from the equator to the pole, the polar column
becomes then heavier than the equatorial by the weight of this
additional layer. A layer of water equal in quantity is therefore
pressed away from the bottom of the column and flows off in the
direction of the equator as an undercurrent, the polar column at
the same time sinking down one foot until equilibrium of the
polar and equatorial columns is restored. Another foot of water
now flows down upon the polar column and another foot of water
is displaced from below, causing, of course, the column to de-
scend an additional foot. The same process being continually
repeated, a constant downward motion of the polar column is
the result. Or perhaps, to express the matter more accurately,
owing to the constant flow of water from the equatorial regions
down the slope, the weight of the polar column is kept always
in excess of that of the equatorial ; therefore the polar column
in the effort to restore equilibrium is kept in a constant state
of descent. Hence he terms it a " vertical " circulation. The
following will show Dr. Carpenter's theory in his own words.

" The action of cold on the surface-water of each polar area will
be exerted as follows :—

" (*a*) In diminishing the height of the polar column as com-
pared with that of the equatorial, so that a lowering of its
level is produced, which can only be made good by a surface-
flow from the latter towards the former.

" (*b*) In producing an excess in the downward *pressure* of the
column when this inflow has restored its level, in virtue of
the increase of specific gravity it has gained by its reduction in
volume ; whereby a portion of its heavy bottom-water is displaced
laterally, causing a further reduction of level, which draws in a
further supply of the warmer and lighter water flowing towards
its surface.

" (*c*) In imparting a downward *movement* to each new surface-
stratum as its temperature undergoes reduction ; so that the

entire column may be said to be in a state of constant descent, like that which exists in the water of a tall jar when an opening is made at its bottom, and the water which flows away through it is replaced by an equivalent supply poured into the top of the jar." (§ 23.)

But if this be his theory, as it evidently is, then the 18 foot-pounds (the amount of work performed by the descent of the water down the slope) comprehends all the work that gravitation can perform on a pound of water in making a complete circuit from the equator to the pole and from the pole back to the equator.

This, I trust, will be evident from the following considerations. When a pound of water has flowed down from the equator to the pole, it has descended 18 feet, and is then at the foot of the slope. Gravity has therefore no more power to pull it down to a lower level. It will not sink through the polar water; for it is not denser than the water beneath on which it rests. But Dr. Carpenter will reply, Although it will not sink through the polar water, it has nevertheless made the polar column heavier than the equatorial, and this excess of pressure forces a pound of water out from beneath and allows the column to descend. Suppose a quantity of water to flow down from the equator so as to raise the level of the polar water by, say, 2 feet. The polar column is now heavier than the equatorial by the weight of this 2 feet of water. The pressure of the 2 feet of water on the polar column will force a quantity of water laterally from the bottom and cause the entire column to descend till the level of equilibrium is restored. In other words, the polar column will sink 2 feet. Now in the sinking of this column work is performed by gravity. A certain amount of work is performed by gravity in causing the water to fall down the slope from the equator to the pole, and, in addition to this, a certain amount is performed by gravity in the vertical descent of the column.

I freely admit this to be sound reasoning, and admit that so much is due to the slope and so much to the vertical descent of the water. In my original way of looking at the problem, I regarded the entire force as due to the slope. In Dr. Carpenter's way of looking at it there is also the slope, but there is, in addition to it, the vertical descent of the water.

But here we come to the most important point, viz. is there the full slope of 18 feet and an additional vertical movement? Dr. Carpenter seems to conclude that there is, and that this vertical force is something in addition to the force which I derive from the slope. And here, I venture to think, is the radical error into which he has fallen in regard to the whole matter.

But let the point at issue be here clearly and distinctly un-
derstood. In my last paper I showed that the difference of tem-
perature between the sea in equatorial and polar regions makes a
difference of level of 18 feet, and that the force of gravity derived
from a slope so small is insufficient to produce the circulation
which Dr. Carpenter supposes. Dr. Carpenter admits this, but
maintains that there is a vertical descent of the water at the
poles, and that the force derived from this vertical descent is the
primum mobile of the circulation,—in short, that though the force
derived from the mere slope is insufficient, nevertheless when we
take into account the force of the vertical descent of the water, a
thing which he supposes I have overlooked, we have a cause per-
fectly sufficient to produce the necessary circulation. This is
the general conclusion arrived at by Dr. Carpenter. Now let it
be observed that I admit, when water circulates from difference
of specific gravity, this vertical movement is just as real a part
of the process as the flow down the slope; but the point which
I maintain is that *there is no additional power derived from this
vertical movement over and above what is derived from the full
slope*—or, in other words, that this *primum mobile*, which he
says I have overlooked, has in reality no existence. Now, if I
manage to establish this point, I trust it will be obvious to the
reader that Dr. Carpenter's theory is untenable.

In Dr. Carpenter's way of looking at the problem, as in mine,
gravity can perform only 18 foot-pounds of work per pound
of water. In his way of treating the problem, the amount
of work performed by the pound of water in first flowing
down the slope and then in descending vertically, when added
together, amounts to exactly 18 foot-pounds. The reason is
obvious; for whatever work is gained by vertical descent is
just so much deducted from the work of descending the slope.
If the pound of water on reaching the pole descends verti-
cally 2 feet, it must have descended only 16 feet in coming
down the slope. Suppose water has flowed down upon the polar
seas so as to restore the level by, say, 2 feet; the polar column
is now too heavy by the extent of 2 feet of water, and the slope
is therefore reduced from 18 feet to 16 feet. Suppose that
while this condition of things remains, a pound of water leaves
the equator and flows down the slope to the pole. In perform-
ing this journey it descends only 16 feet, and consequently only
16 foot-pounds of work is performed. The pressure of the 2
feet of water on the top of the polar column is supposed now to
begin to act; water is pressed out laterally from beneath; the
column descends 2 feet, and equilibrium between the equatorial
and polar columns is restored. The pound of water in this pro-
cess of course descends vertically 2 feet, and consequently 2

foot-pounds of work is performed. But this 2 foot-pounds, added to the 16 foot-pounds, gives simply 18 foot-pounds. Therefore, so far as the amount of work is concerned, it is the same whether the pound of water is supposed to descend through the full slope of 18 feet, or to descend first through a slope of 16 feet and then vertically through the remaining 2 feet.

Perhaps the following diagram will help to make the point still clearer.

Fig. 1.
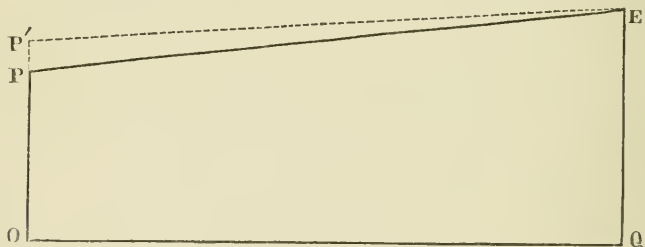
Let P (fig. 1) be the surface of the ocean at the pole, and E the surface at the equator; P O a column of water at the pole, and E Q a column at the equator. The two columns are of equal weight and balance each other; but as the polar water is colder, and consequently denser than the equatorial, the polar column is shorter than the equatorial, the difference in the length of the two columns being 18 feet. The surface of the ocean at the equator E is 18 feet higher than the surface of the ocean at the pole P; there is therefore a slope of 18 feet from E to P. The molecules of water at E tend to flow down this slope towards P. The amount of work performed by gravity in the descent of a pound of water down this slope from E to P is therefore 18 foot-pounds. This represents the state of things in the way in which I have viewed the problem. Dr. Carpenter does not object to all this. It is an essential part of his theory that there is a slope from the equator to the pole. The amount of this slope is, in his way of looking at the question, as in mine, proportional to the difference of density between the polar column and the equatorial; and as he has not called in question my estimate of the difference of density of the two columns, I presume that he admits that the slope is about 18 feet. And of course he will admit that, in the descent of a pound of water down this slope, 18 foot-pounds of work is performed by gravity. But he represents the operation as occurring thus:—As the equatorial column is higher than the polar, when the two are in equilibrium water tends to flow down this slope from the equator to the pole as a surface-current. He assumes this flow to continue till the level

is in whole or in part restored. Take the latter supposition, and suppose that water has flowed down till an addition of 2 feet of water is made to the polar column, and the difference of level, of course, diminished by 2 feet. The surface of the ocean in this case will now be represented by the dotted line P' E, and the slope reduced from 18 feet to 16 feet. Let us then suppose a pound of water to leave E and flow down to P'; 16 foot-pounds will be the amount of work performed. The polar column being now too heavy by the extent of the mass of water P'P 2 feet thick, its extra pressure causes a mass of water equal to P' P to flow off laterally from the bottom of the column. The column therefore sinks down 2 feet till P' reaches P. Now the pound of water in this vertical descent from P' to P has 2 foot-pounds of work performed on it by gravity; this, added to the 16 foot-pounds derived from the slope, gives a total of 18 foot-pounds in passing from E to P' and then from P' to P. This is the same amount of work that would have been performed had it descended directly from E to P. In like manner it can be proved that 18 foot-pounds is the amount of work performed in the descent of every pound of water of the mass P' P. The first pound which left E flowed down the slope directly to P, and performed 18 foot-pounds of work. The last pound flowed down the slope E P', and performed only 16 foot-pounds; but in descending from P' to P it performed the other 2 foot-pounds. A pound leaving at a period exactly intermediate between the two flowed down 17 feet of slope and descended vertically 1 foot. Whatever path a pound of water might take, by the time that it reached P 18 foot-pounds of work would be performed. But no further work can be performed after it reaches P.

But some will ask, in regard to the vertical movement, is it only in the descent of the water from P' to P that work is performed? Water cannot descend from P' to P, it will be urged, unless the entire column P O underneath descend also. But the column P O descends by means of gravity. Why, then, it will be asked, is not the descent of the column a motive power as real as the descent of the mass of water P' P?

Does Dr. Carpenter suppose that motive power is derived from the descent of the polar column P O? Unless he does so, it is difficult to understand what he means by saying that, according to his theory, " the deep efflux of polar water is considered as the *primum mobile* of the General Oceanic Vertical Circulation" (§ 29). Again, unless he considers that the descent of the water below the level of P is a motive power, what grounds can he have for asserting that I have ignored the *primum mobile* of the whole affair? Gravity cannot perform any more work upon the water above the level of P than what is derived from the slope; and

unless he can show that gravity performs work in the way of impelling the water in addition to what I have pointed out, it cannot be said that I have overlooked the influence of any force.

That neither force nor energy can be derived from the mere descent of the polar column P O is easily proved. The reason why the column P O descends is because, in consequence of the mass of water P′ P resting on it, its weight is in excess of the equatorial column E Q. But the force with which the column descends is equal, not to the weight of the column, but to the weight of the mass P′ P ; consequently as much work would be performed by gravity in the descent of the mass P′ P (the two feet of water) alone as in the descent of the entire column P′ O, 10,000 feet in height. Suppose a ton weight is placed in each scale of a balance : the two scales balance each other. Place a pound weight in one of the scales along with the ton weight and the scale will descend. But it descends, not with the pressure of a ton and a pound, but with the pressure of the pound weight only. In the descent of the scale, say, 1 foot, gravity can perform only 1 foot-pound of work. In like manner, in the descent of the polar column, the only work available is the work of the mass P′ P laid on the top of the column. But it must be observed that in the descent of the column from P′ to P, a distance of 2 feet, each pound of water of the mass P′ P does not perform 2 foot-pounds of work ; for the moment that a molecule of water reaches P, it then ceases to perform further work. The molecules at the surface P′ descend 2 feet before reaching P ; the molecules midway between P′ and P descend only 1 foot before reaching P, and the molecules at the bottom of the mass are already at P, and therefore cannot perform any work. The mean distance through which the entire mass performs work is therefore 1 foot. One foot-pound per pound of water represents in this case the amount of work derived from the vertical movement.

That such is the case is further evident from the following considerations. Before the polar column begins to descend, it is heavier than the equatorial by the weight of two feet of water ; but when the column has descended one foot, the polar column is heavier than the equatorial by the weight of only one foot of water ; and, as the column continues to descend, the force with which it descends continues to diminish, and when it has sunk to P the force is zero. Consequently the mean pressure or weight with which the two feet of water P′ P descended was equal to that of a layer of one foot of water ; in other words, each pound of water, taking the mass as a whole, descended with the pressure or weight of half a pound. But a half pound descending two feet performs one foot-pound ; so that whether

we consider the *full pressure acting through the mean distance, or the mean pressure acting through the full distance, we get the same result,* viz. one foot-pound as the work of vertical descent.

Now it will be found, as we shall presently see, that if we cal- culate the mean amount of work performed in descending the slope from the equator to the pole, 17 foot-pounds per pound of water is the amount. The water at the bottom of the mass P P′ moved, of course, down the full slope E P 18 feet. The water at the top of the mass which descended from E to P′ descended a slope of only 16 feet. The mean descent of the whole mass is therefore 17 feet. And this gives 17 foot-pounds as the mean amount of work per pound of water in descending the slope; this, added to the 1 foot-pound derived from vertical descent, gives 18 foot-pounds as the total amount of work per pound of the mass.

I have in the above reasoning supposed 2 feet of water accu- mulated on the polar column before any vertical descent takes place. It is needless to remark that the same conclusion would have been arrived at, viz. that the total amount of work performed is 18 foot-pounds per pound of water, supposing we had con- sidered 3 feet, or 4 feet, or even 18 feet of water to have accu- mulated on the polar column before vertical motion took place.

I have also, in agreement with Dr. Carpenter's mode of repre- senting the operation, been considering the two effects, viz. the flowing of the water down the slope and the vertical descent of the polar column as taking place alternately. In nature, how- ever, the two effects take place simultaneously; but it is needless to add that the amount of work performed would be the same whether the effects took place alternately or simultaneously.

I have also represented the level of the ocean at the equator as remaining permanent while the alterations of level were taking place at the pole. But in representing the operation as it would actually take place in nature, we should consider the equatorial column to be lowered as the polar one is being raised. We should, for example, consider the two feet of water P′ P put upon the polar column as so much taken off the equatorial column. But in viewing the problem thus we arrive at exactly the same results as before, as we shall presently see.

Let P (fig. 2), as in fig. 1, be the surface of the ocean at the pole, and E the surface at the equator, there being a slope of 18 feet from E to P. Suppose now a quantity of water, E E′, say, 2 feet thick, to flow from off the equatorial regions down upon the polar. It will thus lower the level of the equatorial column by 2 feet, and raise the level of the polar column by the same amount. I may, however, observe that the two feet of water in passing from E to P would have its temperature reduced from

80° to 32°, and this would produce a slight contraction.   But as the weight of the mass would not be affected, in order to sim-

Fig. 2.
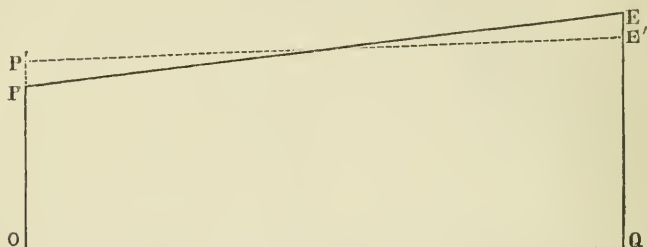
plify our reasoning we may leave this contraction out of consideration.   Any one can easily satisfy himself that the assumption that E E′ is equal to P′ P does not in any way affect the question at issue—the only effect of the contraction being to *increase* by an infinitesimal amount the work done in descending the slope, and to *diminish* by an equally infinitesimal amount the work done in the vertical descent.   If, for example, 16 foot-pounds represent the amount of work performed in descending the slope, and 2 foot-pounds the amount performed in the vertical descent, on the supposition that E′ E does not contract in passing to the pole, then 16·0048 foot-pounds will represent the work of the slope, and 1·9952 foot-pound the work of vertical descent when allowance is made for the contraction.   But the total amount of work performed is the same in both cases.   Consequently, to simplify our reasoning, we may be allowed to assume P′ P to be equal to E E′.

The slope E P being 18 feet, the slope E′ P′ is consequently 14 feet ; the mean slope for the entire mass is therefore 16 feet.   The mean amount of work performed by the descent of the mass will of course be 16 foot-pounds per pound of water. The amount of work performed by the vertical descent of P′ P ought therefore to be 2 foot-pounds per pound.   That this is the amount will be evident thus :—The transference of the 2 feet of water from the equatorial column to the polar disturbs the equilibrium by making the equatorial column too light by 2 feet of water and the polar column too heavy by the same amount of water.   The polar column will therefore tend to sink, and the equatorial to rise till equilibrium is restored.   The difference of weight of the two columns being equal to 4 feet of water, the polar column will begin to descend with a pressure of 4 feet of water ; and the equatorial column will begin to rise with an equal amount of pressure.   When the polar column

has descended 1 foot the equatorial column will have risen 1 foot. The pressure of the descending polar column will now be reduced to 2 feet of water. And when the polar column has descended another foot, P′ will have reached P, and E′ will have reached E; the two columns will then be in equilibrium. It therefore follows that the mean pressure with which the polar column descended the 2 feet was equal to the pressure of 2 feet of water. Consequently the mean amount of work performed by the descent of the mass was equal to 2 foot-pounds per pound of water; this, added to the 16 foot-pounds derived from the slope, gives a total of 18 foot-pounds.

In whatever way we view the question, we are led to the conclusion that if 18 feet represent the amount of slope between the equatorial and polar columns when the two are in equilibrium, then 18 foot-pounds is the total amount of work that gravity can perform upon a pound of water in overcoming the resistance to motion in its passage from the equator to the pole down the slope, and then in its vertical descent to the bottom of the ocean.

But it will be replied, not only does the 2 feet of water P′ P descend, but the entire column P O, 10,000 feet in length, descends also. What, then, it will be asked, becomes of the force which gravity exerts in the descent of this column? We shall shortly see that this force is entirely applied to the overcoming of the resistance offered by gravity to the motion of the water in other parts of the circuit; so that not a single foot-pound of this force goes to overcome cohesion, friction, and other resistances; it is all spent in counteracting the efforts which gravity exerts to stop the current in another part of the circuit. This vertical descent is therefore, not as Dr. Carpenter concludes, some power which I had omitted to take into account in my former determinations.

I shall now consider the next part of the movement, viz. the under or return current from the bottom of the polar column to the bottom of the equatorial. What produces this current? It is needless to say that it cannot be caused directly by gravity. Gravitation cannot directly draw any body horizontally along the earth's surface. The water that forms this current is pressed out laterally by the weight of the polar column, and flows, or rather is pushed, towards the equator to supply the vacancy caused by the ascent of the equatorial column. There is a constant flow of water from the equator to the poles along the surface, and this draining of the water from the equator is supplied by the under or return current from the poles. But the only power which can impel the water from the bottom of the polar column to the bottom of the equatorial column is the pressure of the polar column. But whence does the polar

column derive its pressure? It can only press to the extent
that its weight exceeds that of the equatorial column. That
which exerts the pressure is therefore the mass of water which
has flowed down the slope from the equator upon the polar
column. It is in this case the vertical movement that causes
this undercurrent. The energy which produces this current
must consequently be derived from the 18 foot-pounds result-
ing from the slope; for the energy of the vertical movement,
as has already been proved, is derived from this source; or,
in other words, whatever power this vertical movement may
exert is so much deducted from the 18 foot-pounds derived
from the full slope.

Let us now consider the fourth and last movement, viz. the
ascent of the undercurrent to the surface of the ocean at the
equator. When this cold undercurrent reaches the equatorial
regions, it ascends to the surface to where it originally started
on its circuit. What, then, lifts the water from the bottom of
the equatorial column to its top? This cannot be done directly,
either by heat or by gravity. When heat, for example, is ap-
plied to the bottom of a vessel, the heated water at the bottom
expands and, becoming lighter than the water above, rises
through it to the surface; but if the heat be applied to the sur-
face of the water instead of to the bottom, the heat will not pro-
duce an ascending current. It will tend rather to prevent such
a current than to produce one—the reason being that each
successive layer of water will, on account of the heat applied,
become hotter and consequently lighter than the layer below it,
and colder and consequently heavier than the layer above it.
It therefore cannot ascend, because it is too heavy; nor can it
descend, because it is too light. But the sea in equatorial re-
gions is heated from above, and not from below; consequently
the water at the bottom does not rise to the surface at the equa-
tor in consequence of the heat which it receives. A layer of
water can never raise the temperature of a layer below it to a
higher temperature than itself; and since it cannot do this, it
cannot make the layer under it lighter than itself. That which
raises the water at the equator, according to Dr. Carpenter's
theory, must be the downward pressure of the polar column.
When water flows down the slope from the equator to the pole,
the polar column, as we have seen, becomes too heavy and the
equatorial column too light; the former then sinks and the
latter rises. It is the sinking of the polar column which
raises the equatorial one. When the polar column descends,
as much water is pressed underneath the equatorial column as
is pressed from underneath the polar column. If 1 foot of water
is pressed from under the polar column, a foot of water is

pressed under the equatorial column. Thus, when the polar column sinks a foot, the equatorial column rises by the same extent. The equatorial water continuing to flow down the slope, the polar column descends; a foot of water is again pressed from underneath the polar column and a foot pressed under the equatorial. As foot after foot is thus removed from the bottom of the polar column while it sinks, foot after foot is pushed under the equatorial column while it rises; so by this means the water at the surface of the ocean in polar regions descends to the bottom, and the water at the bottom in equatorial regions ascends to the surface—the effect of solar heat and polar cold continuing, of course, to maintain the surface of the ocean in equatorial regions at a higher level than at the poles, and thus keeping up a constant state of disturbed equilibrium. Or, to state the matter in Dr. Carpenter's own words, "The cold and dense polar water, as it flows in at the bottom of the equatorial column, will not directly take the place of that which has been drafted off from the surface; but this place will be filled by the rising of the whole superincumbent column, which, being warmer, is also lighter than the cold stratum beneath. Every new arrival from the poles will take its place below that which precedes it, since its temperature will have been less affected by contact with the warmer water above it. In this way an ascending movement will be imparted to the whole equatorial column, and in due course every portion of it will come under the influence of the surface-heat of the sun." (Proceedings of the Royal Society, vol. xix. p. 215.)

But the agency which raises the water of the undercurrent up to the surface is the pressure of the polar column. The equatorial column cannot rise directly by means of gravity. Gravity, instead of raising the column, exerts all its powers to prevent its rising. Gravity here is a force acting against the current. It is the descent of the polar column, as has been stated, that raises the equatorial column. Consequently the entire amount of work performed by gravity in pulling down the polar column is spent in raising the equatorial column. Gravity performs exactly as much work in preventing motion in the equatorial column as it performs in producing motion in the polar column; so that, so far as the vertical parts of Dr. Carpenter's circulation are concerned, gravity may be said neither to produce motion nor to prevent it. And this remark, be it observed, applies not only to P O and E Q, but also to the parts P' P and E E' of the two columns. When a mass of water E E', say, 2 feet deep, is removed off the equatorial column and placed upon the polar column, the latter column is then heavier than the former by the weight of 4 feet of water.

Gravity then exerts more force in pulling the polar column down than it does in preventing the equatorial column from rising ; and the consequence is that the polar column begins to descend and the equatorial column to rise. But as the polar column continues to descend and the equatorial to rise, the power of gravity to produce motion in the polar column diminishes, and the power of gravity to prevent motion in the equatorial column increases ; and when P' descends to P and E' rises to E, the power of gravity to prevent motion in the equatorial column is exactly equal to the power of gravity to produce motion in the polar column, and consequently motion ceases. It therefore follows that the entire amount of work performed by the descent of P' P is spent in raising E' E against gravity.

It follows also that inequalities in the sea-bottom cannot in any way aid the circulation ; for although the cold undercurrent should in its progress come to a deep trough filled with water less dense than itself, it would no doubt sink to the bottom of the hollow ; but before it could get out again as much work would have to be performed against gravity as was performed by gravity in sinking it. But whilst inequalities in the bed of the ocean would not aid the current, they would nevertheless very considerably retard it by the obstructions which they would offer to the motion of the water.

We have been assuming that the weight of P' P is equal to that of E E' ; but the mass P' P must be greater than E E', because P' P has not only to raise E E', but it has to impel the undercurrent—to push the water along the sea-bottom from the pole to the equator. So we must have a mass of water, in addition to P' P, placed on the polar column to enable it to produce the undercurrent in addition to the raising of the equatorial column.

It follows also that the amount of work which can be performed by gravity depends entirely on the *difference* of temperature between the equatorial and the polar waters, and is wholly independent of the way in which the temperature may decrease from the equator to the poles. Suppose, in agreement with Dr. Carpenter's idea (' Nature ' for July 6, 1871), that the equatorial heat and polar cold should be confined to limited areas, and that through the intermediate space no great difference of temperature should prevail. Such an arrangement as this would not increase the amount of work which gravity could perform ; it would simply make the slope steeper at the two extremes and flatter in the intervening space. It would no doubt aid the surface-flow of the water near the equator and the poles, but it would retard in a corresponding degree the flow of the water in the intermediate regions. In short, it

would merely destroy the uniformity of the slope without aiding in the least degree the general motion of the water.

I trust, from what has been already stated, it is obvious *that the "* primum mobile*" of Dr. Carpenter has in reality no exist- ence, and that the energy derived from the full slope, whatever that slope may be, comprehends all that can possibly be obtained from gravity. This being the case, there is, according to Dr. Carpen- ter's own admission, not sufficient power to produce the circulation which he assumes.*

If this full slope, as has been proved by direct experi- ment *, be not sufficient to produce even sensible movement of the water from the equator to the pole, a thing which Dr. Car- penter seems to admit †, how much less able is it in addition to this to overcome the resistance to the motion of the water in the horizontal undercurrent and in the vertical descending and ascending currents.

The slope, according to Dr. Carpenter's data, ought to be less than 18 *feet.*

But is there in reality a slope of 18 feet ? Is the equatorial column 18 feet higher than the polar ? My calculations of the differences in the height of the two columns, as has been stated on a former occasion, were made on the assumption that " the temperature of the ocean at the equator decreases at a uniform rate from the surface downwards, which is far from being the case. The rate of decrease is most rapid at the surface, and decreases as we descend. The principal part of the decrease of temperature takes place within no very great depth from the surface ; consequently the greater part of the excess of tempe- rature at the equator over that at the poles affects the sea to no great depth " ‡. It therefore follows that the actual slope must be under 18 feet. I am glad to find that Dr. Carpenter agrees with me, that the principal part of the excess of temperature at the equator is at the surface, and does not extend to any great depth.

" Suppose two basins," he says, " of ocean-water connected by a strait to be placed under such different climatic conditions that the surface of one is exposed to the heating influence of tropical sunshine, whilst the surface of the other is subjected to the extreme cold of the sunless polar winter. The effect of the surface-*heat* upon the waters of the tropical basin will be for the most part limited (as I shall presently show, § 40) to its *upper- most stratum*, and may here be practically disregarded. But the effect of surface-*cold* upon the water of the polar basin will be

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* Phil. Mag. Oct. 1870, p. 254.
† See his footnote to § 25.
‡ Phil. Mag. for October 1870, p. 249.

to reduce the temperature of its *whole* mass below the freezing-point of fresh water" (§ 22). Here Dr. Carpenter not only admits that the greater heating effect of the tropical sun is for the most part confined to the " *uppermost stratum,*" but he goes further, and admits that its effect " may here be *practically disregarded.*" But it seems to me that if the heating of the upper stratum be practically disregarded, then, so far as his theory is concerned, every thing else may be also practically disregarded. For, according to his theory, difference of density is due to difference of temperature; but, on the other hand, the temperature of the sea in intertropical regions differs from that of the sea in polar regions only so far as the warm upper stratum is concerned. The tropical sea is warmer than the polar, just because it receives more heat from the sun than the polar. But the heat of the sun is received on the surface of the sea ; it is " surface-heat." If this warm upper stratum in tropical regions be left out of account, then there is actually no difference whatever between the temperature of the sea in tropical and polar regions. And if there is no difference in temperature, there can be no difference in specific gravity, and consequently nothing, according to Dr. Carpenter's theory, that can possibly move the water. *Cold* is not a something positive imparted to the polar waters giving them motion, and of which the tropical waters are deprived. If we dip one hand in a basin filled with tropical water at 80° and the other in a basin filled with polar water at 32°, referring to our *sensations*, we call the water in the one *hot* and the water in the other *cold* ; but so far as the water itself is concerned, heat and cold simply mean difference in the amounts of heat possessed. Both the polar and the tropical water possess a certain amount of energy in the form of heat, only the polar water does not possess so much of it as the tropical.

But we have more than a mere statement that the excess of heat in equatorial regions is chiefly confined to the upper stratum of the ocean. Dr. Carpenter affords us positive evidence on the subject. From a series of three observations made in the Mediterranean, he found that the superheating produced by the direct action of the sun upon the surface " is almost entirely limited to a stratum *fifty fathoms* deep, the descent of the thermometer being most marked in the first *twenty* fathoms " (§ 40). Fortunately one of the observations was made at a place where the temperature of the water at the surface was as high as 77°, which is within 3° of what I have in my calculations assumed to to be the surface-temperature of the sea in equatorial regions. The following Table will show the rate at which Dr. Carpenter found the temperature to sink from the surface to the depth of 100 fathoms :—

I.		II.	III.
Temperature of surface...		$77 \cdot 0$	$80 \cdot 0$
At 10 fathoms		$71 \cdot 0$	$74 \cdot 0$
20 ,, ...		$61 \cdot 5$	$64 \cdot 5$
30 ,, ...		$60 \cdot 0$	$63 \cdot 0$
40 ,, ...		$57 \cdot 3$	$60 \cdot 3$
50 ,, ...		$56 \cdot 7$	$59 \cdot 7$
100 ,, ...		$55 \cdot 5$	$58 \cdot 5$

Column II. shows the temperature as observed by Dr. Carpenter. If a similar rate of decrease takes place at the equator, which is highly probable, then column III. will show the temperatures at the equator to the depth of 100 fathoms. Dr. Carpenter says that in the Atlantic he " found that, after passing through the heated surface-layer, there was a slow nearly uniform descent of temperature down to the ' stratum of intermixture,' in which there was another sudden drop of 10°." We may therefore in our calculations assume the decrease of temperature to be uniform below 100 fathoms. We have now a means of determining with more accuracy than before the actual height of the surface of the ocean at the equator above that at the poles. Taking, as before, Muncke's Table of the rate of expansion of seawater, it turns out that the height of the equatorial column above the polar amounts to little more than 8 feet. But to give Dr. Carpenter's theory full justice, I shall assume the temperature of polar water from the surface to the bottom to be not 32°, but three degrees lower, viz. 29°. This will make the slope from the equator to the pole about 9 feet, or one half what I had made it in my former paper. The distance from the equator to the poles is 32,758,000 feet. But to simplify calculations, let us take the distance in round numbers at 31,500,000 feet. This reduction of the distance is, of course, so far in favour of Dr. Carpenter's theory. We have here an inclined plane 31,500,000 feet in length and 9 feet in height. The height of the plane to its length is therefore as 1 to 3,500,000. According to the principle of the inclined plane, the force of gravity tending to move a pound of water down this plane is $\frac{1}{3,500,000}$ of a pound, or $\frac{1}{500}$ of a grain. Were water a perfect fluid and could move without any resistance, a pound of water under the pressure of $\frac{1}{500}$ of a grain would flow down from the equator to the pole with an accelerated motion, and on reaching the pole it would have acquired the same velocity as it would have done supposing it had fallen vertically through a distance of 9 feet. It would reach the pole with an amount of energy in the form of motion equal to 9 foot-pounds. Water, however, is not a perfect fluid, but offers

considerable resistance to motion. Now, if the resistance offered by water to motion exceeds the pressure of $\frac{1}{500}$ of a grain per pound, gravity will be unable to cause the water to flow down the incline under consideration ; for were it to do so, the work of the resistances would exceed the work of gravity—a thing impossible. If the resistances of the water to motion equalled $\frac{1}{500}$ of a grain per pound, the entire 9 foot-pounds of energy would be expended in carrying the pound of water from the equator to the pole, and no energy would remain to cause the water to descend to the bottom of the ocean, to return to the equator as an under current, and then to ascend to the surface. If we assume, what certainly must be the case, that the total amount of resistance offered to the motion of the water in the under-current and in the two vertical currents equals the resistance offered in the surface-current, then the force of the resistance to the pound of water, if motion is to take place, must be under $\frac{1}{1000}$ of a grain. For as we have only 9 foot-pounds of energy for the entire circuit, $4\frac{1}{2}$ foot-pounds would be spent on the surface-current, and the other $4\frac{1}{2}$ foot-pounds would go to produce the under and two vertical currents. But if the resistances exceeded $\frac{1}{1000}$ of a grain on the pound of water, the work of the resistances in the entire circuit would exceed the work of gravity, and consequently no motion would be possible. M. Dubuat, as has already been stated [*], found that water remains motionless on an incline of 1 in 1,000,000 ; but such an inclination gives for the force of gravity $\frac{1}{143}$ of a grain. This shows that the resistance to the motion of the water amounts to at least $\frac{1}{143}$ of a grain. But before circulation can take place, according to Dr. Carpenter's theory, the resistance of the water would require to be only one seventh of that amount, viz. $\frac{1}{1000}$ of a grain. We are therefore led to the conclusion that it is absolutely impossible that difference of specific gravity can produce even motion of the waters of the ocean, far less such a circulation as that assumed by Lieut. Maury and Dr. Carpenter.

I have not been able to find any direct determinations as to the work of the resistances in the flow of water ; but the above show indirectly that it must far exceed the work of gravity. Canon Moseley has lately been investigating the question of the resistance of fluids by means of his new method, which he had applied with such remarkable success to glacier-motion [†]. By this method he has been able to determine the amount of the internal work of resistance of the films of water to the flowing of each film over the surface of the next in succession ; and there is little doubt but he will be able to settle the question

* Phil. Mag. October 1870, p. 251.
† Phil. Mag. for September 1871, p. 184.

whether or not 9 foot-pounds is sufficient to carry a pound of water from the equator to the pole as a surface-current, and back from the pole to the equator as an undercurrent.

Dr. Carpenter must either show that the total amount of *work* that can be performed by gravity on a pound of water in its entire circuit from the equator to the pole and back to where it started exceeds 9 foot-pounds, or else that the mean *force* of the resistances to the motion of the pound of water is under $\frac{1}{1000}$ of a grain. Unless he can do this, he is not warranted to conclude that the General Oceanic Circulation which he advocates is even a physical possibility, far less a physical necessity.

But this force of $\frac{1}{500}$ of a grain, infinitesimal as it is, holds true only in regard to the water at the surface. The power of gravity to produce motion decreases rapidly as we proceed downwards; at a short distance below the surface it may be practically disregarded.

In determining whether a certain slope be sufficient to produce motion of the water, there is a distinction of the utmost importance which must be borne in mind; viz. we must consider whether the slope be caused by difference of specific gravity or by some other agency, such, for example, as the wind heaping up the water. Suppose the ocean to be the same in density from the equator to the poles, and that by some means or other the water should be so heaped up or raised at the equator as to produce a difference of level of 18 feet between the equator and the poles. It is more than probable that the force of gravity would in this case be sufficient to cause such a motion of the water as would restore the level of the ocean; for the ocean would not be in a state of equilibrium when the water stood 18 feet higher at the equator than at the poles, because the equatorial column would exceed the polar by the weight of this 18 feet of water; consequently the entire weight of this mass would be employed in restoring the level. But when the rise of 18 feet at the equator is the result of specific gravity, the sea is in perfect equilibrium and no pressure whatever is exerted; the only thing in such a case that could restore the level or produce motion is that almost infinitesimal tendency that the molecules at the surface have to roll down the slope.

I cannot but think that, on this account, Dr. Colding has very much overestimated the power of gravity in the case of the Gulf-stream. He states that, in order to impel the waters of the Gulf-stream through the Straits of Florida, a slope of 9 feet from the Gulf of Mexico to St. Augustine is a necessity. But difference of specific gravity affords only 6 feet of slope; the additional three feet he assumes to be derived from the heaping up of the water in the Gulf of Mexico under the influence of the

trade-winds. From these two causes he derives the necessary slope of 9 feet. But here a new agent is brought into play of an entirely different character from that of the slope, viz. the pressure of a mass of water 3 feet deep. In consequence of this the weight of a column of water in the Gulf of Mexico exceeds that of a column in the North Atlantic by three feet of water. Each square foot of the cross section of the Gulf-stream is thus subjected to a hydrostatic pressure of about 190 pounds urging the water forward. The amount of work which this pressure is capable of performing in a given time is proportionate to the superficial area of the column ; but the superficial area of the column is equal to that of the entire Gulf of Mexico ; we have, therefore, in this three feet of water heaped up in the Gulf a power sufficient of itself to produce a very considerable current through such a narrow strait as that of Florida. But were the 9 feet of slope wholly the result of difference of specific gravity, the waters of the Gulf and the Atlantic would be in perfect equilibrium, and there would then be no pressure from behind impelling the water forward through the strait. The only thing which could then have any influence in producing a current would be the force which gravity would exert on the molecules at the surface, tending to cause them to roll down the slope, and which in the case of a slope so small would be but trifling. In the case of Dr. Carpenter's oceanic circulation, this force, as we have just seen, amounts to only one five-hundredth of a grain on a pound of water.

But there are other causes which tend to reduce the slope still further :—(1) There can be no permanent current while the full slope of 9 feet remains. A permanent current requires a state of constant disturbed equilibrium between the equatorial and polar columns ; and this again, as has been shown, necessarily implies a permanent reduction of slope. (2) Although the polar waters are colder than the intertropical, yet they are not so salt, and of course are, on this account, not so heavy. (3) In intertropical regions evaporation is in excess of precipitation ; but in temperate and polar regions precipitation is in excess of evaporation. This, to a certain extent, tends to lower the level of the sea in intertropical regions, and to raise the level of the sea in temperate and polar regions; so much water is removed from the surface of the ocean in intertropical regions and poured down as rain or snow upon the surface of the ocean in temperate and polar regions. In short, if there be any difference of level between the surface of the ocean in equatorial and polar regions, it must be trifling, indeed so trifling as to be absolutely incapable of producing the motion supposed.

It cannot be urged as an objection to what has been advanced

that I have determined simply the amount of the force acting on the water at the surface of the ocean and not that on the water at all depths—that I have estimated the amount of work which gravity can perform on a given quantity of water at the surface, but not the total amount of work which gravity can perform on the entire ocean. This will not do as an objection, because the surface of the ocean is the place where the greatest difference of temperature, and consequently of density, exists between the equatorial and polar waters, and therefore it is here that gravity exerts its greatest force. And if gravity be unable to move the water at the surface, it is much less able to do so under the surface. So far as the question at issue is concerned, any calculations as to the amount of force exerted by gravity at various depths are needless.

Dr. Carpenter, in proof of his theory, adduces the fact that at a depth of 2000 fathoms or so the ocean, from the poles to the equator, has a temperature not above $32°$. He concludes, and that justly, that this low temperature at the equator is evidence of the existence of an undercurrent from the poles to the equator. But he maintains, like Maury and others, that such an undercurrent could not result from the action of winds on the surface of the ocean, but must be due to difference of specific gravity. This opinion, I find, is supported by no less authorities than Sir William Thomson and Professor Stokes. But with all due deference to these great physicists, I am, for the following reasons, unable to accept such an opinion. Suppose, for argument's sake, that the Gulf-stream is caused by the winds, and that it flows into the Arctic seas. It is evident that the water which is thus being constantly carried from the intertropical to the Arctic regions must in some way or other find its way back to the equator ; in other words, there must be a re-turn-current equal in magnitude to the direct current. Now the question to be determined is what path must this return-current take ? It appears to me that it will take the *path of least resistance*, whether that path may happen to be at the surface or under the surface. But that the path of least resistance will, as a general rule, lie at a very considerable distance below the surface is, I think, evident from the following considerations. At the surface the general direction of the currents is opposite to that of the return-current. The surface-motion of the water in the Atlantic is from the equator to the pole ; but the return-current must be from the pole to the equator. Consequently the surface-currents will oppose the motion of any return-cur-rent unless that current lie at a considerable depth below the surface-currents. Again, the winds, as a general rule, blow in an opposite direction to the course of the return-current, because,

according to supposition, the winds blow in the direction of the surface-currents. From all these causes the path of least resistance to the return-current will, as a general rule, not be at the surface, but at a very considerable depth below it.

It is maintained also that the winds cannot produce a vertical current except under some very peculiar conditions. We have already seen that, according to Dr. Carpenter's theory, the vertical motion is caused by the water flowing off the equatorial column, down the slope, upon the polar column, thus destroying the equilibrium between the two by diminishing the weight of the equatorial column and increasing the weight of the polar column. In order that equilibrium may be restored, the polar column sinks and the equatorial one rises. Now must not the same effect occur, supposing the water to be transferred from the one column to the other, by the influence of the winds instead of by the influence of gravity ? The vertical descent and ascent of these columns depend entirely upon the difference in their weights, and not upon the nature of the agency which makes this difference. So far as difference of weight is concerned, 2 feet of water, say, propelled down the slope from the equatorial column to the polar by the winds, will produce just the same effect as though it had been propelled by gravity. If vertical motion follows as a necessary consequence from a transference of water from the equator to the poles by gravity, it follows equally as a necessary consequence from the same transference by the winds ; so that one is not at liberty to advocate a vertical circulation in the one case and to deny it in the other.

Dr. Carpenter, as well as Maury, maintains that currents produced by the winds cannot extend to any great depth. It is certainly true that sudden commotions caused by storms do not generally extend to great depths. Neither will winds of short continuance produce a current extending far below the surface. But prevailing winds which can produce such immense surface-flow as that of the great equatorial currents of the globe and the Gulf-stream, which follow definite directions, must communicate their motion to great depths, unless water be frictionless, a thing which it is not. Suppose the upper layer of the ocean to be forced on by the direct action of the winds with a constant velocity of, say, four miles an hour, the layer immediately below will be dragged along with a constant velocity somewhat less than four miles an hour. The layer immediately below this second layer will in turn be also dragged along with a constant velocity somewhat less than the one above it. The same will take place in regard to each succeeding layer, the constant velocity of each layer being somewhat less than the one immediately above it, and greater than the one below it. The question to be determined

is, at what depth will all motion cease? I presume that at present we have not sufficient data for properly determining this point. The depth depends upon the amount of molecular resistance offered by the water to motion—in other words, on the amount of the shearing-force of the one layer over the other. The fact, however, that motion imparted to the surface will extend to great depths can be easily shown by direct experiment. If a constant motion be imparted to the surface of water, say, in a vessel, motion will ultimately be communicated to the bottom, no matter how wide or how deep the vessel may be. The same effect will take place whether the vessel be 5 feet deep or 500 feet deep.

But it does not follow that though a current, such as the Gulf-stream, impelled by the influence of the winds may not extend to any great depth, the water which it conveys may not descend to the bottom of the ocean. The water of the Gulf-stream is much salter than the water of the North Atlantic. Now by the time that this salt water reaches the shores of, say, Iceland or the Shetlands, its temperature will be so far reduced that though it may be 10° or 12° higher than the polar water underneath, still, in consequence of its greater saltness, it may be actually denser, and on this account may sink to the bottom and displace the polar water. In short, the difference in density between the salt water of the Gulf-stream, by the time that it reaches our shores, and the cold polar water is so infinitesimal that it can exercise but little or no influence in determining the position of the current. The fact pointed out by Professor Wyville Thomson must also be borne in mind, viz. that the Gulf-stream flows into an almost closed basin.

The Gibraltar Current.

If difference of specific gravity fails in accounting for the currents of the ocean in general, it certainly fails in a still more decided manner in accounting for the Gibraltar current. The existence of the submarine ridge crossing between Capes Trafalgar and Spartel affects currents resulting from difference of specific gravity in a manner which does not seem to have suggested itself to Dr. Carpenter. The pressure of water and other fluids is not like that of a solid, say, the weight in the scale of a balance, simply a downward pressure. Fluids press downwards like the solids, but they also press laterally. The pressure of water is hydrostatic. If we fill a basin with water or any other fluid, the fluid remains in perfect equilibrium, provided the sides of the basin be sufficiently strong to resist the pressure. The Mediterranean and Atlantic, up to the level of the submarine ridge referred to, may be regarded as huge

basins, the sides of which are sufficiently strong to resist all pressure.   It follows that, however denser the water of the Mediterranean may be than the water of the Atlantic, it is only the water above the level of the ridge that can possibly exercise any influence in the way of disturbing equilibrium, so as to cause the level of the Mediterranean to stand lower than that of the Atlantic.   Suppose both basins empty, and dense water to be poured into the Mediterranean, and water less dense into the Atlantic, until they are both filled up to the level of the ridge ; still the heavier water in the one basin exercises no in-- fluence in raising the level of the lighter water in the other basin, the entire pressure being borne by the sides of the basins. But if we continue to pour in water till the surface is raised, say, 1 foot, above the level of the ridge, then there is nothing to resist the lateral pressure of this 1 foot of water in the Mediteranean but the counter pressure of the 1 foot in the Atlantic.   But as the Mediterranean water is denser than the Atlantic, this 1 foot of water will consequently exert more pres- sure than the 1 foot of water of the Atlantic.   We must there- fore continue to pour in more water into the Atlantic until its lateral pressure equals that of the Mediterranean.   The two seas will then be in equilibrium, but the surface of the Atlantic will of course be at a higher level than the surface of the Mediterra- nean.   The difference of level will be proportionate to the differ- ence in density of the waters of the two seas.   But here we come to the point of importance.   In determining the difference of level between the two seas, or, what is the same thing, the difference of level between a column of the Atlantic and a column of the Mediterranean, we must take into consideration *only the water which lies above the level of the ridge.*   If there be 1 foot of water above the ridge, then there is a difference of level proportionate to the difference of pressure between the 1 foot of water of the two seas.   If there be 2 feet, 3 feet, or any number of feet of water above the level of the ridge, the differ- ence of level is proportionate to the 2 feet, 3 feet, or whatever number of feet there may be of water above the ridge.   If, for example, 13 should represent the density of the Mediterranean water and 12 the density of the Atlantic water, then if there were 1 foot of water in the Mediterranean above the level of the ridge, there would require to be 1 foot 1 inch of water in the Atlantic above the ridge in order that the two might be in equilibrium.   The difference of level would therefore be 1 inch. If there were 2 feet of water, the difference of level would be 2 inches; if 3 feet, the difference would be 3 inches, and so on.   And this would follow, no matter what the actual depth of the two basins might be ; the water below the level of the ridge exer- cising no influence whatever on the level of the surface.

Now, in determining the actual difference of level between the Mediterranean column and the Atlantic, we must leave out of account the water under the level of the ridge. This point must be so obvious to every one familiar with hydrostatics that I need not go into further detail in the matter.

Taking Dr. Carpenter's own data as to the density of the Mediterranean and Atlantic waters, what, then, is the difference of density ? The submarine ridge comes to within 167 fathoms of the surface ; say, in round numbers, to within 1000 feet. What are the densities of the two basins down to the depth of 1000 feet ? According to Dr. Carpenter there is little, if any, difference. His own words on this point are these :—"A comparison of these results leaves no doubt that there is an excess of salinity in the water of the Mediterranean above that of the Atlantic ; but that this excess *is* slight in the surface-water, whilst somewhat greater in the deeper water " (§ 7). "Again, it was found by examining samples of water taken from the surface, from 100 fathoms, from 250 fathoms, and from 400 fathoms respectively, that whilst the *first two* had the *characteristic temperature and density of Atlantic water,* the last two had the characteristics and density of Mediterranean water" (§ 13). Here, at least to the depth of 100 fathoms or 600 feet, there is little difference of density between the waters of the two basins. Consequently down to the depth of 600 feet there is nothing to produce any sensible disturbance of equilibrium. If there be any sensible disturbance of equilibrium, it must be in consequence of differ- ence of density which may exist between the depths of 600 feet and the surface of the ridge. We have nothing to do with any difference which may exist between the water of the Medi- terranean and the Atlantic below the ridge ; the water in the Mediterranean basin may be as heavy as mercury below 1000 feet ; but this can have no effect in disturbing equilibrium. The water, to the depth of 600 feet, being of the same density in both seas, the length of the two columns acting on each other is therefore reduced to 400 feet—that is, to that stratum of water lying at a depth of from 600 to the surface of the ridge 1000 feet below the surface. But, to give Dr. Carpenter's theory full justice, we shall take the Mediterranean stratum at the density of the deep water of the Mediterranean, which he found to be about 1·029, and the density of the Atlantic stratum at 1·026. The difference of density between the two columns is therefore ·003. Consequently, if the height of the Mediterranean column be 400 feet, it will be balanced by the Atlantic column of 401·2 feet ; the difference of level between the Mediterranean and the Atlantic cannot therefore be more than 1·2 foot. The total amount of work that can be performed by gravity in the case

of the Gibraltar current is little more than 1 foot-pound per pound of water, an amount of energy totally inadequate to pro- duce the current.

The Baltic Current.

The entrance to the Baltic Sea is in some places not over 50 or 60 feet deep. It follows, therefore, from what has already been proved in regard to the Gibraltar current, that the influ- ence of gravity must be even still less in causing a current in the Baltic strait than in the Gibraltar strait.

Dr. Carpenter's Objections to my Estimate of the absolute amount of Heat conveyed by the Gulf-stream.

After giving a full exposition of his theory, he closes with the following remarks :—

" Having thus fortified my own position by showing that the power I have invoked has a real existence and a most extended and varied operation instead of being a figment of my own imagination (as Mr. Croll represents it), I shall venture to attack the stronghold of my adversaries by showing that the Gulf-stream at the point of its greatest ' glory' can by no means claim the heating-power which they assign to it " (§ 39). He then proceeds to criticise the data on which my estimate was formed. But surely in my paper on the subject I must not have expressed myself with sufficient clearness, seeing that Dr. Carpenter has on several important points misunderstood me.

He begins with the following quotation from my paper in the Philosophical Magazine, February 1870, p. 82. " From an ex- amination of the published sections some years ago," says Mr. Croll, " I came to the conclusion that the total quantity of water conveyed by the stream is probably equal to that of a stream fifty miles broad and 1000 feet deep, flowing at the rate of four miles an hour "*. He then assumes that all my con- clusions regarding the enormous amount of heat conveyed by the stream, and from which my inferences as to his theory were drawn, were based upon this estimate of the volume of the stream. This, it is true, was the volume adopted in my former estimate

* The above gives 5,575,680,000,000 cubic feet per hour as the volume of the stream. Professor Wyville Thomson, in reference to this, says, " I see no reason whatever to believe this calculation to be excessive" (' Nature' for July 27, 1871, p. 252). Dr. Colding, in his recent elaborate memoir " On the Gulf-stream," estimates the volume at 5,760,000,000,000 cubic feet per hour. Maury, as we have seen, estimates it at 6,165,700,000,000 cubic feet, and Herschel at 7,359,900,000,000 cubic feet. But in my paper the calculations were made on the assumption that the volume is only 2,787,840,000,000 cubic feet.

of the heating-power of the stream; but it will be seen, by re-
ferring to a page a little further on than the one from which he
quotes, viz. to page 89, that, as Mr. Findlay had maintained
that I had doubled the actual volume*, I reduced it to one
half of my former estimate. My object for doing so was that I
might be enabled to show that, so far as my general conclu-
sions regarding the influence of the Gulf-stream were con-
cerned, it is a matter of indifference whether I adopt Mr.
Findlay's estimate or my own. The inference drawn in my
former paper regarding Dr. Carpenter's theory is therefore based
on the assumption that the volume of the stream is only one
half what he had supposed I had assumed it to be.

He states that, in estimating the volume of the stream, I had
taken the velocity at the surface for the mean velocity. I am
unable to perceive on what grounds he was led to such a con-
clusion. I did not state that the Gulf-stream is anywhere fifty
miles broad and 1000 feet deep, and flowing at the rate of four
miles an hour. What I stated was that the quantity of water
conveyed is probably *equal* to that of a stream fifty miles broad,
1000 feet deep, and flowing of course at every point from the
surface to the bottom at the rate of four miles an hour; but I
never anticipated that any one would conclude from this that I
imagined the Gulf-stream actually flowed with the same velocity
at every point from the surface to the bottom. Such an opinion
as this I never held nor ever could have held. Most certainly
the velocity of the Gulf-stream, like that of currents in general,
diminishes from the surface, or from near to the surface, down-
wards.

He states that I have also overestimated the mean tempera-
ture of the stream. I have taken the mean temperature of the
stream at the moment of its leaving the Gulf of Mexico at 65°;
and he says that this is too high an estimate. He states also
that what has misled me on this point is, that my esti-
mate seems based on the assumption that in proceeding from
above downwards the temperature descends uniformly between
the different points of observation (§ 40). And in order to
show that this is not the law of decrease of temperature down-
wards, he presents us with the Table of observations made in
the Mediterranean, to which reference has already been made.
And from this Table he concludes, and no doubt justly, that
the rate of decrease is greatest near to the surface of the stream
and diminishes as we proceed downwards. But supposing I had
adopted Dr. Carpenter's Table as representing the law accord-
ing to which the temperature of the water diminishes from the
surface of the stream downwards, I am still unable to perceive

* Proceedings of the Royal Geographical Society, vol. xiii. p. 233.

how this would have given a lower mean temperature than 65°. We might, by means of Dr. Carpenter's method, arrive at a pretty accurate estimate of the mean temperature of the *cross section* of the stream ; and it is quite probable that the mean temperature of the cross section would be found to be under 65°. But although the mean temperature of the cross section should be below 65°, it does not follow on this account that the mean temperature of the *water flowing through this cross section* must be below that temperature. It is perfectly obvious that the mean temperature of the mass of water flowing through the cross section in a given time must be much higher than the mean temperature of the cross section itself. Perhaps Dr. Carpenter has overlooked this fact, and hence the reason why he has arrived at so low an estimate of the mean temperature of the stream.

The reason why the temperature of the water must be higher than that of the cross section is this :— It is in the upper half of the section where the high temperature exists ; but as the velocity of the stream is far greater in its upper half than in its lower half, the greater portion of the water passing through this cross section is water of high temperature. If Dr. Carpenter would take this fact into consideration and again go over his calculations, I feel persuaded he would arrive at the conclusion that the mean temperature of the Gulf-stream at the moment of leaving the Gulf is not under 65°.

But, be all this as it may, let us assume that the volume of the stream is but one half what he supposes I took it to be in my calculations, and also that the *quantity of heat* conveyed per pound of water is but $12\frac{1}{2}$ units instead of 25 units, as in my estimate ; in other words, let us assume the mean temperature of the stream to be $52\frac{1}{2}°$ instead of 65°. Surely these are concessions that will satisfy not only Dr. Carpenter, but every one else. Let us consider now what are the consequences to which we are still led in regard to his theory of a general interchange of equatorial and polar water independently of the Gulf-stream.

The area from which Dr. Carpenter derives his heat is the area of the Atlantic, extending from the equator to the tropic of Cancer, including the Caribbean Sea and the Gulf of Mexico. By referring to my last paper (Phil. Mag. for Oct. 1870, pp. 255–257) it will be seen that, taking the volume of the Gulf-stream at one-half of what Dr. Carpenter supposes I estimated it to be in my last paper, and assuming that the quantity of heat conveyed into the Atlantic in temperate regions by means of his general circulation is equal to that conveyed by the Gulf-stream (whose heat he assumes does not pass beyond the temperate regions), the amount of heat removed from the Torrid zone and

transferred over into the Temperate zone would be so enormous as to make the Atlantic in the temperate regions much warmer than the Atlantic in the torrid region.   The relative quantities, as will be seen by referring to my last paper, are these : 1124 parts would represent the amount of heat in the temperate region of the Atlantic, and 570 parts the amount in the torrid region.

But supposing we now take the mean temperature of the Gulf-stream at $52\frac{1}{2}°$ instead of $65°$, in other words, that the amount of heat conveyed is but $12\frac{1}{2}$ units instead of 25 units, and assuming that the amount conveyed by his general movement is even not more than that conveyed by the Gulf-stream at this re-duced rate, what are still the consequences ? By referring to the data afforded in my last paper (p. 257), it can be easily calculated that 202·5 parts of heat will be removed from the torrid region and transferred to the temperate region.   Consequently the amount of heat possessed by the Atlantic in torrid regions will be 772 parts, and that possessed by the Atlantic in temperate regions will be as much as 940 parts.

This conclusion alone, I would venture to think, is decisive proof that, even supposing such a motion of the ocean as that for which Dr. Carpenter contends were physically possible, still the amount of heat conveyed by means of his general circu-lation must, as regards the Atlantic, be perfectly trifling in com-parison with that conveyed by means of the Gulf-stream.   But this is not all ; it proves also that the quantity of heat conveyed by aërial currents and all other means put together is but trifling compared with that conveyed by the Gulf-stream.

Dr. Petermann has, by an entirely different line of argument, shown in the most clear and convincing manner that the abnor-mally high temperature of the north-western shores of Europe and the seas around Spitzbergen is owing entirely to the Gulf-stream, and not to any general circulation such as that advocated by Dr. Carpenter. From a series of no fewer than 100,000 obser-vations of temperature in the North Atlantic and in the Arctic seas, he has been enabled to trace with accuracy on his charts the very footsteps of the heat in its passage from the Gulf of Mexico up to the shores of Spitzbergen [*].

Mr. A. G. Findlay's Objections.

At the conclusion of the reading of Dr. Carpenter's paper,

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[*] *Der Golfstrom und Standpunkt der thermometrischen Kenntniss des nord-atlantischen Oceans und Landgebiets im Jahre* 1870. A translation of this important memoir of Dr. Petermann's, along with one or two others which have lately appeared in his *Geographische Mittheilungen,* would be a boon to English readers.

Mr. Findlay rose to make some observations, and, among other things, made the following remarks :—

" When, by the direction of the United States Government ten or eleven years ago, the narrowest part of the Gulf-stream was examined, figures were obtained which shut out all idea of its ever reaching our shores as a heat-bearing current. In the narrowest part, certainly not more than from 250 to 300 cubic miles of water pass per diem.   Six months afterwards that water reaches the banks of Newfoundland, and nine or twelve months afterwards the coast of England, by which time it is popularly supposed to cover an area of 1,500,000 square miles.   The proportion of the water that passes through the Gulf of Florida will not make a layer of water more than 6 inches thick per diem over such a space.   Every one knows how soon a cup of tea cools; and yet it is commonly imagined that a film of only a few inches in depth, after the lapse of so long a time, has an effect upon our climate.   There is no need for calculations; the thing is self-evident."

About two years ago Mr. Findlay objected to the conclusions which I had arrived at regarding the enormous heating-power of the Gulf-stream on the ground that I had overestimated the volume of the stream.   He stated that its volume was only about the half of what I had estimated it to be.   To obviate this objection in future, in my last paper on the heating-power of the stream, published in the February Number of the Philosophical Magazine for 1870, I reduced the volume to one half of my former estimate.   But taking the volume at this low estimate, it was nevertheless found that the quantity of heat conveyed into the Atlantic through the straits of Florida by means of the stream was equal to about *one fourth* of all the heat received from the sun by the Atlantic from the latitude of the Strait of Florida up to the Arctic Circle.

Mr. Findlay, in his paper read before the British Association, stated that the volume of the stream is somewhere from 294 to 333 cubic miles per day ; but in his remarks made at the close of Dr. Carpenter's address, he states it to be not greater than from 250 to 300 cubic miles per day.   I am unable to reconcile any of those figures with the data from which he appears to have derived them.   He stated in his paper to the British Association that " the Gulf-stream at its outset is not more than $39\frac{1}{2}$ miles wide, and 1200 feet deep."   " From all attainable data," he says, " he computes the mean annual rate of motion to be 65·4 miles per day ; but as the rate decreases with the depth, the mean velocity of the whole mass does not exceed 49·4 miles per day.   But when he speaks of the mean velocity of the Gulf-stream being so and so, he must refer to the mean velocity at

some particular place. This is evident; for the mean velocity entirely depends upon the sectional area of the stream. The place where the mean velocity is 49·4 miles per day must be the place where it is $39\frac{1}{2}$ miles broad and 1200 feet deep; for he is here endeavouring to show us how small the volume of the stream actually is. Now, unless the mean velocity refers to the place where he gives us the breadth and depth of the stream, his figures have no bearing on the point in question. But a stream $39\frac{1}{2}$ miles broad and 1200 feet deep has a sectional area of 8·97 square miles, and this, with a mean velocity of 49·4 miles per day, will give 443 cubic miles of water. The amount, according to the estimate taken in my last paper, is 459 cubic miles per day; it therefore exceeds Mr. Findlay's estimate by only 16 cubic miles.

Mr. Findlay, so far as I am aware, does not consider that I have overestimated the mean temperature of the stream. He states (Brit. Assoc. Report, 1869, p. 160) that between Sand Key and Havanna the Gulf-stream is about 1200 feet deep, and that it does not reach the summit of a submarine ridge, which he states has a temperature of 60°. It is evident, then, that the bottom of the stream has a temperature of at least 60°, which is within 5° of what I regard as the mean temperature of the mass. But the surface of the stream is at least 17° above this mean. Now, when we consider that it is at the upper parts of the stream, the place where the temperature is so much above 65°, that the motion is greatest, it is evident that the mean temperature of the entire moving mass must, according to Mr. Findlay, be considerably over 65°. It therefore follows, according to his own data, that the Gulf-stream conveys into the Atlantic an amount of heat equal to one fourth of all the heat which the Atlantic, from the latitude of the Straits of Florida up to the Arctic regions, derives from the sun.

But even supposing we were to halve Mr. Findlay's own estimate, and assume that the volume of the stream is equal to only 222 cubic miles of water per day instead of 443, still the amount of heat conveyed would be equal to one eighth part of the heat received from the sun by the Atlantic. But would not the withdrawal of an amount of heat equal to one eighth of that received from the sun greatly affect the climate of the Atlantic? Supposing we take the mean temperature of the Atlantic at, say, 56°; this will make its temperature 295° above that of space. Extinguish the sun and stop the Gulf-stream, and the temperature ought to sink 295°. How far, then, ought the temperature to sink, supposing the sun to remain and the Gulf-stream to stop? Would not the withdrawal of the stream cause the temperature to sink some 30°? Of course, if the Gulf-stream were withdrawn and every thing else were to remain the same, the tem-

perature of the Atlantic would not actually remain 30° lower than at present; for heat would flow in from all sides and partly make up for the loss of the stream. But nevertheless 30° represents the amount of temperature maintained by means of the heat from the stream. And this, be it observed, is taking the volume of the stream at a lower estimate than even Mr. Findlay would be willing to admit. Mr. Findlay says that, by the time the Gulf-stream reaches the shores of England, it is supposed to cover a space of 1,500,000 square miles. "The proportion of water that passes through the Straits of Florida will not make," says Mr. Findlay, "a layer of water more than 6 inches thick per diem over such a space." But a layer of water 6 inches thick cooling 25° will give out 579,000 foot-pounds of heat per square foot. If, therefore, the Gulf-stream, as Mr. Findlay asserts, supplies 6 inches per day to that area, then every square foot of the area gives off per day 579,000 foot-pounds of heat. The amount of heat received from the sun per square foot in latitude 55°, which is not much above the mean latitude of Great Britain, is 1,047,730 foot-pounds per day, taking, of course, the mean of the whole year; *consequently this layer of water gives out an amount of heat equal to more than* one half *of all that is received from the sun.* But assuming that the stream should leave the half of its heat on the American shores and carry to the shores of Britain only $12\frac{1}{2}$° of heat, still we should have 289,500 foot-pounds per square foot, which notwithstanding *is more than equal to* one fourth *of that received from the sun.* If an amount of heat so enormous cannot affect climate, then what can possibly do it?

I shall just allude to one other erroneous notion which prevails in regard to the Gulf-stream; but it is an error which I by no means attribute to either Mr. Findlay or to Dr. Carpenter. The error to which I refer is that of supposing that when the Gulf-stream widens out to hundreds of miles, as it does before it reaches our shores, its depth must on this account be much less than when it issues from the Gulf of Mexico. Although the stream may be hundreds of miles in breadth, there is no necessity why it should be only 6 inches, or 6 feet, or 60 feet, or even 600 feet in depth. It may just as likely be 6000 feet deep as 6 inches.

The reason why such diversity of opinion prevails in regard to Ocean-currents.

In conclusion I venture to remark that more than nine tenths of all the error and uncertainty which prevail, both in regard to the cause of ocean-currents and to their influence on climate, is due, not, as is generally supposed, to the intrinsic difficulties of the

subject, but rather to the defective methods which have hitherto been employed in its investigation—that is, in not treating the subject according to the rigid methods adopted in other departments of physics. What I most particularly allude to is the disregard paid to the modern method of determining the amount of effects in *absolute measure*.

But let me not be misunderstood on this point. I by no means suppose that the *absolute quantity* is the thing always required for its own sake. It is in most cases required simply as a means to an end; and very often that end is the knowledge of the *relative* quantity. Take, for example, the Gulf-stream. Suppose the question is asked, to what extent does the heat conveyed by that stream influence the climate of the North Atlantic? In order to the proper answering of this question, the principal thing required is to know what proportion the amount of heat conveyed by the stream into the Atlantic bears to that received from the sun by that area. We want the *relative proportions* of these two quantities. But how are we to obtain them? The only way we can obtain them is by determining first the *absolute* quantity of each. We must first measure each before we can know how much the one is greater than the other, or, in other words, before we can know their relative proportions. In regard to the absolute amount of heat received from the sun by a given area at any latitude, we have the means of determining this with tolerable accuracy. The same cannot be done with equal accuracy in regard to the amount of heat conveyed by the Gulf-stream, because the volume and mean temperature of the stream are not known with certainty. Nevertheless we have sufficient data to enable us to fix upon such a maximum and minimum value to these quantities as that every one will admit the truth must lie somewhere between them. In order to give full justice to those who maintain that the Gulf-stream exercises but little influence on climate, and to put an end to all further objections as to the uncertainty of my data, I have taken a minimum to which none of them surely can reasonably object, viz. that the volume of the stream is not over 230 cubic miles per day, and the heat conveyed per pound of water not over $12\frac{1}{2}$ units. Calculating from these data, we find that the amount of heat carried into the North Atlantic is equal to one eighth of all the heat received from the sun by that area. There are, I presume, few who will not admit that the actual proportion is much higher than this, probably as high as 1 to 3, or 1 to 4. But who, without adopting the method I have pursued, could ever have come to the conclusion that the proportion was even 1 to 8? He might have guessed it to be 1 to 100 or 1 to 1000, but he never would have guessed it to be 1 to 8. Hence the reason why the great in-

fluence of the Gulf-stream as a heating-agent has been so much underestimated.

The same remarks apply to the gravitation-theory of the cause of currents. Viewed simply as a theory it looks very reasonable. There is no one acquainted with physics but will admit that the tendency of the difference of temperature between the equator and the poles is to cause a surface-current from the equator towards the poles, and an under-current from the poles to the equator. But before we can prove that this tendency does actually produce such currents, another question must be settled, viz. is this force which tends to produce the motion sufficiently strong to overcome the resistance of the water to go into motion? Dr. Carpenter has never attempted to prove that it is; he has simply taken it for granted. But when we apply the method to which I refer, and determine the absolute amount of the force resulting from the difference of specific gravity, we find it to be not that powerful thing which the advocates of the gravitation-theory suppose it to be, but a force so infinitesimal as not to be worthy of being taken into account when considering the causes which produce currents.

XXX. *The Solid Crust of the Earth cannot be thin.*
By Archdeacon Pratt, *M.A., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

M. DELAUNAY considers that the Solid Crust of the Earth may be thin—and that, if it be so, the fluid nucleus moves exactly as the crust does in precession and nutation, in consequence of the viscosity of the fluid and the extreme *slowness* of the motion—and that, in consequence, the late Mr. W. Hopkins's argument from precession that the crust is thick, at least 800 or 1000 miles, does not stand examination. I have already forwarded two letters to you on this subject; and now ask you to publish a third, to show why the *slowness* of the motion has nothing to do with the question.

The particles of the earth's mass have much more to undergo than performing this slow motion. They have during the twenty-four hours of the earth's rotation to undergo a varying strain which no fluid, even viscous, could sustain. The force producing the slow motions of precession and nutation is the result of the near balance of a multitude of forces acting throughout the mass, almost equal and opposite to each other. Suppose of two fluid molecules in contact one only is acted on by a minute

force equal to one millionth of a grain weight. No doubt its neighbour, by friction and viscosity, might be induced to move on with it. But suppose one were acted on by a force of one whole grain *plus* one millionth of a grain, and the other by one grain in the opposite direction. If the particles were rigidly connected the pair would move on as before, as the difference of forces is the same. But if they are fluid particles, however viscous, they would part company. Now this represents the condition to which the particles of the earth's mass are perpetually being subjected, as I propose now to show.

2. Let S be the mass of the sun, c its distance from the earth's centre; let the axis of x be drawn through the sun from that centre as origin, the plane of xy being the ecliptic.

The attraction of the sun on any particle of the earth's mass is S $\div$ (distance)^2 towards the sun. The disturbing force of the sun on this particle relatively to the earth's centre is the resultant of this force and a force S $\div c^2$ acting on the particle parallel to the axis of x, away from the sun. When these two forces are resolved parallel to the axes of coordinates, very small quantities of the second order being neglected, and S $\div c^3$ put $= \mu$, and forces tending *from* the earth's centre reckoned *positive*, the three disturbing forces on the particle parallel to the axes are

$$2\mu \cdot x, \quad -\mu \cdot y, \quad -\mu \cdot z.$$

3. I will take the first of these first, viz. $2\mu x$ parallel to x. On the sun-side of the plane yz it is always positive; on the opposite side it is always negative. The aggregate of the forces on all the particles on the one side will produce a resultant force drawing the earth's mass towards the sun, and the resultant on the opposite side will be an equal force drawing it in an opposite direction. These two equal and opposite forces would act at the same point in the plane yz (viz. the earth's centre) if the earth's mass were symmetrically arranged about its centre. Owing to the slight deviation from that arrangement, the first resultant force will act at a point extremely near the centre, but not at the centre; and the other force parallel to it at an equal distance on the opposite side; so that the two forces will produce a mechanical " couple," with a very short " arm," the moment of which will be a very small quantity, although the two forces are not so small. I at present assume the earth's particles to be all rigidly connected together. The couple will tend to make the earth revolve round a diameter at right angles to the plane of the couple and lying in the plane yz.

The effect of the sun's action will therefore be to put the earth's mass in a state of *tension* in the direction of the line joining the earth and sun, tending every instant to separate the

two unsymmetrical hemispheroids into which the plane yz separates it. The disturbing forces parallel to y and z will, on the other hand, produce *compression* in directions parallel to those lines. These also will produce two couples, of small moment, tending to make the earth rotate round diameters perpendicular to their planes and lying in the planes xz and xy respectively.

4. It is not difficult to calculate these forces and their moments about the axes. I will do this, first, supposing the earth's mass to be *homogeneous*.

Let l be the longitude of the sun, ω the obliquity of the ecliptic, a the *mean* radius of the earth, ϵ the ellipticity. Then the equation to the surface is

$$x^2 + y^2 + z^2 = a^2(1+\epsilon) - 2\epsilon(x \sin l \sin \omega + y \cos l \sin \omega + z \cos \omega)^2$$
$$= a^2(1+\epsilon) - 2\epsilon(\mathrm{D}x + \mathrm{E}y + \mathrm{F}z)^2,$$

where

$$\mathrm{D} = \sin l \sin \omega, \quad \mathrm{E} = \cos l \sin \omega, \quad \mathrm{F} = \cos \omega \,;$$

the square of the ellipticity I shall neglect;

$$\therefore \ x = -2\mathrm{D}(\mathrm{E}y + \mathrm{F}z)\epsilon \pm \sqrt{a^2(1+\epsilon) - y^2 - z^2 - 2\epsilon(\mathrm{E}y + \mathrm{F}z)^2}.$$

It will be convenient to put for a time

$$y + 2\mathrm{E}\mathrm{F}z\epsilon = \mathrm{Y}, \text{ and } a\left(1 + \epsilon(\tfrac{1}{2} - \mathrm{F}^2)\right) = \alpha.$$

Then the values of x will be

$$x = -2\mathrm{D}(\mathrm{E}\mathrm{Y} + \mathrm{F}z)\epsilon \pm (1 + \mathrm{E}^2\epsilon)\sqrt{1 + 2\epsilon(\mathrm{F}^2 - \mathrm{E}^2)(\alpha^2 - z^2) - \mathrm{Y}^2},$$

and the limiting values of Y in the plane yz will be

$$\mathrm{Y} = \pm\left(1 + \epsilon(\mathrm{F}^2 - \mathrm{E}^2)\right)\sqrt{\alpha^2 - z^2},$$

and the limiting values of z will be $\pm\alpha$.

As it will be useful, I will write down the value of x^2:

$$x^2 = (1 + 2\epsilon\mathrm{F}^2)(\alpha^2 - z^2) - (1 + 2\epsilon\mathrm{E}^2)\mathrm{Y}^2 \mp 4\mathrm{D}(\mathrm{E}\mathrm{Y} + \mathrm{F}z)\sqrt{\alpha^2 - z^2 - \mathrm{Y}^2}.$$

The whole force parallel to x on the hemispheroid nearest the sun

$$= 2\mu \iiint \rho \, dz \, dy \, dx . x = \mu\rho \iint dz \, d\mathrm{Y} \, x^2 = \tfrac{1}{4}\mu\rho\pi a^4,$$

neglecting the small term in ϵ compared with the first. This is the force of *tension* sustained at every instant by the section of the earth by the plane yz, there being an equal force drawing the opposite half in the opposite direction. The force of *compression* perpendicular to the planes xz, xy will be half this.

5. I must now find the moment of the forces parallel to x about the axes of y and z. I will here once for all say that those moments will be reckoned *positive* which tend to turn the earth about the axis of x from the axis of y to the axis of z, about y from z to x, about z from x to y.

Moment about axis of z of forces parallel to x acting on the sun-side of the plane yz

$$= -2\mu\rho \iiint dz\, dy\, dx \,.\, xy = -\mu\rho \iint dz\, dY x^2 (Y - 2EF\epsilon z)$$

$$= \tfrac{8}{15}\, \mu\rho DEa^5\epsilon \text{ after all reductions.}$$

There will be an equal moment, and in the same direction, on the opposite side. Hence

Moment about axis of z of forces parallel to $x = \tfrac{16}{15}\pi\mu\rho DEa^5\epsilon$.

Similarly we obtain by integration,

Moment about axis of y of forces parallel to $x = -\tfrac{16}{15}\pi\mu\rho DFa^5\epsilon$,

,,	,,	x	,,	,,	$y = -\tfrac{8}{15}\pi\mu\rho EFa^5\epsilon$,
,,	,,	z	,,	,,	$y = +\tfrac{8}{15}\pi\mu\rho DEa^5\epsilon$,
,,	,,	x	,,	,,	$z = +\tfrac{8}{15}\pi\mu\rho EFa^5\epsilon$,
,,	,,	y	,,	,,	$z = -\tfrac{8}{15}\pi\mu\rho DFa^5\epsilon$.

Compounding these,

Moment of all the forces about axis of $x = \quad 0$,

,,	,,	,,	$y = -\tfrac{8}{5}\pi\mu\rho DFa^5\epsilon$,
,,	,,	,,	$z = +\tfrac{8}{5}\pi\mu\rho DEa^5\epsilon$.

6. If the earth's mass be regarded as *heterogeneous*, as it is, and arranged in layers of small ellipticity increasing from the centre towards the surface, it is easy to see that these expressions are true of each of these layers—which equals the *difference* between two homogeneous spheroids, if $\rho d(a^5\epsilon)$ be put for $\rho a^5\epsilon$. Hence for the whole earth,

Force which tends to separate the parts divided by the plane yz

$$= \tfrac{1}{4}\pi\mu \int \rho \frac{d\,.\,a^4}{da}\, da.$$

Moment of the forces about $x = \quad 0$,

,,	,,	$y = -\tfrac{8}{5}\pi\mu DF \int \rho \dfrac{d\,.\,a^5\epsilon}{da}\, da$,
,,	,,	$z = +\tfrac{8}{5}\pi\mu DE \int \rho \dfrac{d\,.\,a^5\epsilon}{da}\, da$.

Substituting for D, E, F, these two moments are, by mecha-

nics, equal to one moment

$$-\frac{8}{5}\pi\mu\sin l\sin\omega\sqrt{1-\sin^2 l\sin^2\omega}\int\rho\frac{d\cdot a^5\epsilon}{da}\,da$$

about a diameter in the plane yz, making an angle ψ with the axis of y such that

$$\tan\psi=-\cot\omega\div\cos l.$$

7. In the accompanying diagram let Q, P be the poles of the ecliptic and earth on the mean spherical surface of the earth, O the earth's centre, Q P q the solstitial colure, V the vernal equinox, V G the equator cutting the plane yz in G. Then P Q G equals l, the sun's longitude, Q P $=\omega$, the obliquity. Then, by spherical trigonometry, as PG = 90°, we have
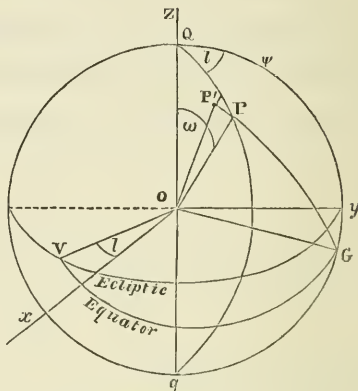
$$\cos l=\cos \text{PQG}=-\cot \text{QG}\cdot\cot \text{QP},$$

$$\therefore \tan \text{QG}=-\cot\omega\div\cos l=\tan\psi.$$

Hence $\text{QG}=\psi$, and OG, the line in which the plane of the equator intersects the plane yz, is the diameter about which the disturbing force of the sun tends to make the earth revolve. And the angular accelerating force round that diameter

$$=-\frac{8}{5}\pi\mu\sin l\sin\omega\sqrt{1-\sin^2 l\sin^2\omega}\int\rho\frac{d\cdot a^5\epsilon}{da}\,da\div\frac{8}{15}\pi\int\rho\frac{d\cdot a^5}{da}\,da,$$

the divisor being the moment of inertia of the earth,

$$=-3\mu f\sin l\sin\omega\sqrt{1-\sin^2 l^2\,\omega},$$

where

$$f=\int\rho\frac{d\cdot a^5\epsilon}{da}\,da\div\int\rho\frac{d\cdot a^5}{da}\,da.$$

This will generate an angular velocity about OG in the time dt

$$=3\mu f\sin l\sin\omega\sqrt{1-\sin^2 l\sin^2\omega}\cdot dt.$$

But the earth revolves round its axis OP. The combined effect of these two angular velocities is, that it will revolve round

another axis OP′, such that P′ is in the great circle GP, and sin PP′ : sin P′G : : angular velocity about G : to angular velocity about P. Hence PP′ being extremely small, and n being the earth's angular velocity of rotation,

$$PP' = 3\mu f \sin l \sin \omega \sqrt{1 - \sin^2 l \sin^2 \omega}\, dt \div n.$$

The instant the mass tends to revolve round a new axis OP′, close to OP the principal axis of greatest moment (as the earth's axis is), the centrifugal force of the unsymmetrical parts will cause OP to move up to OP′; that is, not merely is there a new axis in space about which the earth revolves, but the earth's own axis of figure moves up to it.

Now resolve this small space PP′ perpendicular to and along PQ, and integrate, to get the whole effect during one revolution of the sun (that is, of the earth about the sun), by which $l = n't$, n' being the earth's mean motion about the sun. Hence, since

$$\sec \psi = \sqrt{1 - \sin^2 l \sin^2 \omega} \div \sin \omega \cos l,$$

space moved over along PQ

$$= \int PP' \cos QPP' = - \int PP' \cos \psi \div \sin l$$
$$= -3\mu f \int \cos l \sin^2 \omega\, dl \div nn' = 0, \text{ from } l = 0 \text{ to } l = 2\pi.$$

Hence, though there will be nutation of the axis, the value of PQ or the obliquity will, in a whole revolution, be unaltered.

Space moved over by P at right angles to PQ

$$= \int PP' \sin QPP' = \int PP' \sin \psi \sin l = 3\mu f \sin \omega \cos \omega \int \sin^2 l\, dl \div nn'$$
$$= 3\pi \mu f \sin \omega \cos \omega \div nn' = 3\pi f n' \sin \omega \cos \omega \div n, \therefore \mu = n'^2.$$

This, measured along the ecliptic, $= 3\pi f n' \cos \omega \div n$, and coincides with the usual expression for solar annual precession. An exactly similar expression is true for the moon's action, supposed to move in the ecliptic; n'' being the mean motion of the moon about the earth, the lunar monthly precession will be

$$3\pi f n'' \cos \omega \div n.$$

Hence the whole

$$\text{Annual Precession} = 3f \frac{n'^2 + n''^2}{nn'} 180°.$$

8. The calculations of the Figure of the Earth* make $f = 0{\cdot}003136$; also $n' \div n = 365{\cdot}26$, $n'' \div n' = 36526 \div 2732$.

* See my ‘Mechanical Philosophy,’ chapter on Figure of the Earth.

These, when substituted, make

<p style="text-align:center">Annual Precession = $51''\cdot36$.</p>

9. Were the material of the earth not highly rigid, the par-
ticles would yield to the strain which I have shown would be per-
petually coming upon them, going through its changes every
twelve hours, and would describe reentering curves on the sur-
faces of infinitesimal ellipsoids. The result would be that the
strain could not be transmitted through the mass, and preces-
sion and nutation would not occur.

Observation makes the annual precession $= 50''$. And the
result of the above calculations, viz. $51''\cdot36$, is *very* near this.
This near coincidence shows how highly rigid the earth's mass
must be.

10. But it may be thought that if the interior is fluid, and
the solid part only of a certain thickness t, the crust to be moved
being a less mass than the whole earth, the observed precession
might still coincide with what calculation would bring it out, as
the action of the sun and moon on the solid crust would be less,
and the pressure of the fluid would perhaps compensate for any
difference. This I propose now to examine. I will, however,
previously, in this paragraph, calculate the quantity f for a shell
or solid crust.

Let a and a be the mean radii of the outer and inner surfaces
of the crust. In this case

$$f = \int_a^a \rho \, \frac{d \cdot a^5 \epsilon}{da} \, da \div \int_a^a \rho \, \frac{d \cdot a^5}{da} \, da.$$

This ratio, it will be easily seen, is a larger quantity than when
$a = 0$, or for the whole earth. For the larger a is the larger is ϵ;
and therefore the elementary terms in the numerator of f for
only the crust are the largest of all the terms of f for the whole
earth; and they correspond term by term with the terms in the
denominator. Hence for the crust f is larger than for the whole
earth.

Suppose, as usual, that $\rho = Q \sin qa \div a$; then by the Figure
of the Earth,

$$\epsilon = H \frac{\left(1 - \dfrac{3}{q^2 a^2}\right) \tan qa + \dfrac{3}{qa}}{\tan qa - qa}, \quad \frac{qa}{\tan qa} = 1 - z,$$

where H is independent of a.

The advocates for a thin crust consider 100 miles sufficient.
This is one fortieth of the radius, the square of which may be
neglected. Call, then, t the thickness, and expand;

$$\therefore f = \int_{a-t}^{a} \rho \frac{d \cdot a^5 \epsilon}{da} da \div \int_{a-t}^{a} \rho \frac{d \cdot a^5}{da} da = \rho \frac{d \cdot a^5 \epsilon}{da} t \div \rho \frac{d \cdot a^5}{da} t$$

$$= \frac{\epsilon}{5}\left(5 + \frac{a}{\epsilon}\frac{d\epsilon}{da}\right) = \frac{\epsilon}{5}\left\{\frac{zq^2a^2}{3z - q^2a^2} + \frac{3z - q^2a^2}{z}\right\}$$

after reduction : ϵ is here the ellipticity of the crust. Now by the Figure of the Earth,

$$q^2a^2 \div z = 1\cdot 5, \quad q^2a^2 = 6\cdot 054, \quad \epsilon = \frac{1}{295} ;$$

$$\therefore f, \text{ for the crust, } = 0\cdot 00375.$$

This is larger than it is for the whole earth : see above.

11. Now I must consider the action of the sun and moon on the fluid, and so on the crust by pressure. I shall suppose that at the instant under consideration the fluid is, as M. Delaunay supposes it to be, arranged and moving just as if it were part of the solid mass of the earth, but capable of communicating pressure. The forces acting on the fluid are the centrifugal force arising from the earth's rotation, the attraction of the sun and moon, and the attraction of the crust and fluid. These last and the centrifugal force cannot disturb the position of the axis, owing to the symmetry of the crust. Nor can any constant pressure, the same on every part of the inner surface of the crust. Hence I shall reject all constant terms in the expression for p at the surface. The equation of equilibrium, then, taking only the sun at present, is

$$dp = \rho(2\mu x dx - \mu y dy - \mu x dx)$$
$$= \tfrac{1}{2}\rho\mu d(2x^2 - y^2 - z^2) = \tfrac{1}{2}\mu\rho d(3x^2 - r^2).$$

In calculating the effect of the pressure on the crust, I shall neglect in p all terms depending on ϵ, because otherwise I should be introducing terms depending on the square. This is clear ; for the smallness of the moment of the pressure arises from the near symmetry of the figure, and therefore the near balance of pressure on opposite sides of the centre. Now considering ρ to be a function of r, we have, as $x \div r$ is independent of r,

$$p = \frac{\mu}{2}\left(\frac{3x^2}{r} - 1\right)\int \rho d \cdot r^2 = \frac{3\mu G}{2a^2} x^2 \text{ at the surface,}$$

putting G for the definite integral, and rejecting the constant term. The cosine of the angle which the normal at any point of the inner surface of the crust makes with the axis of x is

$$\frac{dz}{dx} \div \sqrt{1 + \frac{dz^2}{dx^2} + \frac{dz^2}{dy^2}} = \frac{x}{a}\left(1 - \frac{\epsilon}{2}\right) + \frac{2D}{a}(Dx + Ey + Fz)\epsilon$$
$$- \frac{x(Dx + Ey + Fz)^2}{a^3}.$$

This, multiplied by p, is the pressure parallel to the axis of x; and the projection on the plane yz of an element of the surface $= dy\, dz$. The pressure at the point xyz parallel to x will act in the same line as that at the corresponding point when x is taken negative. We may add the two effects together; this will strike out terms in which odd powers of x occur, and will simplify the formula. Thus,

Moment of pressure parallel to x about z

$$= -\iint dz\, dy\, p\, \frac{4\mathrm{D}}{a}\, (\mathrm{E}y + \mathrm{F}z)\epsilon \left(1 - \frac{x^2}{a^2}\right) . y$$

$$= -\frac{6\mu \mathrm{GD}\epsilon}{a^3} \iint dz\, dy \left(x^2 - \frac{x^4}{a^2}\right)(\mathrm{E}y^2 + \mathrm{F}zy),$$

after reduction

$$= -\tfrac{1}{4}\pi\mu \mathrm{G}\epsilon \mathrm{D}\mathrm{E}a^3.$$

So of the others :—

Moment about y of pressure parallel to $x = \quad \tfrac{1}{4}\pi\mu \mathrm{G}\epsilon \mathrm{D}\mathrm{F}a^3,$

$\quad$,, $\qquad x \qquad$,, $\qquad$,, $\qquad y = -\tfrac{3}{16}\pi\mu \mathrm{G}\epsilon \mathrm{E}\mathrm{F}a^3,$

$\quad$,, $\qquad z \qquad$,, $\qquad$,, $\qquad y = \quad \tfrac{9}{16}\pi\mu \mathrm{G}\epsilon \mathrm{D}\mathrm{E}a^3,$

$\quad$,, $\qquad x \qquad$,, $\qquad$,, $\qquad z = \quad \tfrac{3}{16}\pi\mu \mathrm{G}\epsilon \mathrm{E}\mathrm{F}a^3,$

$\quad$,, $\qquad y \qquad$,, $\qquad$,, $\qquad z = -\tfrac{9}{16}\pi\mu \mathrm{G}\epsilon \mathrm{D}\mathrm{F}a^3.$

These six moments are the same as

Moment about axis of $x = \quad 0,$

$\qquad$,, $\qquad$,, $\qquad y = -\tfrac{5}{16}\pi\mu \mathrm{G}\epsilon \mathrm{D}\mathrm{F}a^3,$

$\qquad$,, $\qquad$,, $\qquad z = +\tfrac{5}{16}\pi\mu \mathrm{G}\epsilon \mathrm{D}\mathrm{E}a^3.$

These are precisely analogous to the result of paragraph 5. Hence putting

$$\frac{5}{16}\, \mathrm{G}\epsilon a^3 \text{ for } \frac{8}{5}\, \mathrm{D}\, \frac{d \cdot a^5\epsilon}{da}\, da = 8f\, \mathrm{D}a^4 t, \text{ or } \frac{5}{128}\, \frac{\mathrm{G}\epsilon}{\mathrm{D}ta} \text{ for } f,$$

D being the density of the surface, the effect of the fluid pressure on the crust will be to *increase* f by

$$\frac{5}{128}\, \frac{\mathrm{G}\epsilon}{\mathrm{D}ta} = \frac{5\epsilon}{64t \sin qa} \int_0^a \sin qa\, da = \frac{5\epsilon(1 - \cos qa)}{64 qa \sin qa}\, \frac{a}{t}$$

$$= \frac{5\epsilon}{64}\, \frac{\sec qa - 1}{qa \tan qa}\, \frac{a}{t} \text{ nearly.}$$

By the Figure of the Earth, $qa \tan qa = -2$, $\sec qa = -1\cdot2872$. Then, putting $t = 100$ miles, f is increased by $0\cdot012115$.

The effect of the fluid would thus be greatly to increase the precession. This increase would, moreover, take place if we allowed any reduction in the pressure in consequence of viscosity. The friction of the fluid in contact with the solid crust would be a minute quantity not comparable with the effect of the fluid pressure.

12. Hence, finally, from paragraphs 8, 10, and 11,

Calculated Precession for solid earth . . . $= 51''\cdot36$

Ditto for solid crust $= \dfrac{3750}{3136}\ 51''\cdot36 = 61''\cdot41$

Ditto for effect of fluid $= \dfrac{12115}{3136}\ 51''\cdot36 = 198''\cdot41$

$\underline{}\ 259''\cdot82$

From both causes, the thinness of the crust and the action of the fluid pressure, the Precession would be increased by a quantity so perceptible as to show that, even if at any instant the crust and the fluid revolve alike, this could not continue. It also confirms the previous conclusion in paragraph 9, that the earth's mass can be neither elastic nor fluid to any great extent, but must be highly rigid—as Sir William Thomson has already shown from independent calculations regarding the tides.

If the advocates for a thin crust say that the pressure at any point in the interior is so enormous, owing to the weight of materials above the point, as to hold the particles firmly together and enable even the fluid portions to sustain unaffected the strain to which they are perpetually being subjected, I say that the interior must have lost its fluid properties, and is actually solid, and that with such a high degree of rigidity as to bear and communicate the strain which comes upon it; any yielding would be fatal to this effect. Pressure has in that case turned what was liquid at a certain temperature into a solid rigid mass, now at a lower temperature; and the result is, that the interior is solid, not fluid; which is all I am contending for.

13. I have shown in this communication that the *slowness* of the motions of precession and nutation can in no way justify the supposition that the interior of the earth, were it a viscous fluid, would move exactly as the solid crust does; for in the production of those motions, slow as they may be, the mass has to undergo a strain which no fluid, even if viscous, could sustain.

It may be thought, perhaps, that nevertheless the latter part of my paper may allow us to suppose that the interior is fluid, and the crust, though solid, yet not rigid, but possessing a certain degree of suppleness, so that, though the precession would,

as I show above, be considerably larger than the observed amount (50″·1) if the crust were rigid, it would be reduced to the observed quantity owing to its yielding in some measure to the disturbing forces. This, however, is answered by Sir William Thomson's investigation on the Rigidity of the Earth. He shows that if the material of the earth yielded as little as even a globe of steel or glass would under the action of the disturbing forces of the sun and moon, the effect on both the tides and the precession would be very perceptible, and both would be *less* than for a rigid earth. This we know is not the case.. I think, therefore, that geologists must submit to the verdict that the crust of the earth is very thick, if not solid to the centre, and must be content with the idea that there are local seas of lava in the crust itself to account for volcanic phenomena.

<div align="right">John H. Pratt.</div>

Calcutta, August 5, 1871.

XXXI. *On the Action of Light on Chlorine and Bromine.* By Dr. E. Budde, *of the University of Bonn*[*].

IN the course of an investigation on the combustion of explosive gaseous mixtures and on catalytic action in which I am engaged, I frequently had to apply the well-known proposition of Favre and Silbermann and Clausius, according to which the molecules of most elementary gases consist of two atoms; and in the experimental prosecution of deductions drawn from this hypothesis I have arrived at some rather remarkable results with regard to chlorine and bromine. Assuming the hypothesis to be true (and certainly it is as probable as Avogadro's theorem and the equality of specific heats of gases for equal volumes), it naturally leads to the conclusion that the so-called combination of two elementary gases must in general be preceded by a splitting up of their molecules into isolated atoms, and that consequently such a combination will be promoted by any influence which induces a separation from each other of *equal* atoms without hindering the combination of the unequal ones.

Now it is known that in chlorine, through insolation, there is induced a higher degree of chemical activity. This fact might be accounted for in two ways—(1) by assuming that light increases the attraction between the atoms Cl of chlorine and the atoms k of the respective other body, and (2) by assuming that light *tends* to resolve, or actually *does* resolve, the chlorine molecule into its constituent atoms. Of these two hypotheses the former does not appear very probable—the less so, as, for instance, in the

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action of light on chloride of silver we observe a direct severing of the connexion between Ag and Cl,—while the latter, *à priori* at least, does not provoke any objections. Adopting it for the moment, there remains the open question, whether we are to imagine that the rays of the light actually split up the molecules Cl^2, or only that they loosen their bonds of union, so as to facilitate a complete separation by the affinity to Cl of the foreign atom k. Theoretically speaking, the one kind of action would appear as probable as the other; in fact I see no reason against their coexistence. But the former one, if it be true, must have some remarkable consequences, which may easily be tried by experiment. Hence I was induced to pursue it more closely.

I assume that light in general diminishes the force uniting Cl with Cl, and that occasionally, through the cooperation of light and internal motion (heat), a molecule, Cl^2, is actually split up into its constituents $Cl^1 + Cl^1$.

If the chlorine is mixed, say, with hydrogen, it is easily seen that these free atoms will readily combine with the atoms of the latter, and may even (if their number rapidly increase) induce an explosion. But if the chlorine be pure, then the isolated atoms Cl will fly about like independent molecules between the undecomposed molecules Cl^2; occasionally two of them will meet and reunite, so that finally, in case of a constant intensity of light and constant temperature, there will be reached a state of dynamical equilibrium, where in any given moment the number of molecules split up will exactly equal the number of molecules reformed. In other words, insolated chlorine always contains a certain proportion of free atoms (increasing, no doubt, with the intensity of the light); and this, together with Avogadro's theorem, leads to the conclusion that free chlorine, through insolation, *increases in specific volume,* the more so the more intense the active portion of the rays falling upon it.

Moreover it is extremely probable that the reunion of isolated chlorine-atoms involves a production of heat; if so, the rays of high refrangibility would do a kind of work which ultimately leads to a (be it ever so small) stationary increase of temperature. Hence the final conclusion to be drawn from our assumption is, that *chlorine when exposed to " chemical" rays must expand, but when brought back into the dark recontract to its original normal volume.*

This proposition I have tested by experiment and found correct.

The apparatus used was a Leslie's differential thermometer, which was filled with chlorine and illuminated with various parts of a solar spectrum produced by means of a glass prism. The bulbs were of 5–6 centims. diameter; the connecting tube had a bore of about 1 millimetre. In the first experiments the bulbs

were closed by simply sealing them up, subsequently by means of soldered-up glass stopcocks. Concentrated sulphuric acid previously saturated with chlorine served as index-fluid. The viscosity of this liquid renders the thermometer rather unsensitive; yet I preferred it to any other on account of its stability and its small vapour-tension. Only in a few confirmatory experiments I used chloride of carbon, CCl^4, as indicator, in order to show that the phenomenon observed was not caused by the action of the sulphuric acid, and also to form an idea on the duration of the reaction (*vide infra*). The bulbs were so placed that either of them could be exposed to any portion of the solar spectrum; their distance from the prism varied from 1 to 2 metres; and their shadows, according to their position, covered from one third to one sixth of the visible part of the spectrum. The index was illuminated with gas-light, and its position observed with respect to one of the cross wires of a telescope.

Equilibrium of temperature having been established, the cross wire was made to coincide with the end of the index, the light then made to act, and the variation in the position of the index observed and estimated. Let us designate one of the bulbs by A, the other by B, and call positive any motion of the index from A towards B; then the results of one of the series of observations may be stated as follows:—

Illumination of		Displacement of index.
A.	B.	
Ultra-red + red.	Dark.	$+\frac{1}{2}$ to 1 millim.
Red + yellow.	,,	$+\frac{1}{2}$ millim.
Blue + violet.	,,	5 to 6 millims.
Trace of blue + violet } + ultra-violet.	Dark.	6 to 7 ,,
Ultra-violet.	,,	4 ,,
Dark.	Red.	$-\frac{1}{4}$ or less.
,,	Violet + adjoining rays.	-5 millims.
Red and yellow.	Ultra-violet, limit.	+ not measurable.
The whole of the red } end of the spectrum.	Violet end of the spectrum.	} -2 to 3 millims.
Dark.	Dark.	$+\frac{1}{3}$ millim.*

The experiments were repeated several times with substantially the same results; only it occasionally occurred that the sulphuric acid would not move at all; but in such cases it would no more obey a slight increase of temperature; and when the

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* Instead of 0; as the series of observations extended over about twenty minutes, this small difference is easily accounted for by a slight difference in temperature of the bulbs.

apparatus was emptied and refilled, the anomaly in its working disappeared. The displacements were rather slow, and sometimes took several minutes to attain their maximum ; but this is satisfactorily accounted for by the viscosity of the fluid ; for (1) the same slowness of motion is observed when the instrument is used as a common differential thermometer in the dark; and (2) when chloride of carbon, $C Cl^4$, was substituted for sulphuric acid, the index attained its final position in a few seconds. Hence it may be assumed that the state of equilibrium in insolated chlorine very quickly follows any change in the (chemical) intensity of the illumination. The light I used was sunlight of only middling brightness, which was reflected by a bad mirror and decomposed by a small glass prism ; the " ultra-violet " above-mentioned only encompasses the rays not absorbed by glass.

Incontestably proved by these experiments is the fact of the existence of a substance which *apparently* behaves to " *actinic* " as most other known bodies do to " *thermic* " rays. I have little doubt that in my experiments it really was the chemical individual *chlorine* which produced the effect. In order to get further conviction, control experiments were made.

(1) An ordinary but very sensitive differential thermometer (with an indicator probably consisting of coloured spirit) was treated quite as the former one ; in the more refrangible part of the spectrum no increase of temperature could be observed.

(2) A differential thermometer charged with carbonic acid and ether as an indicator behaved in the blue light like the one with air and spirit ; according to Tyndall, the contents of the bulbs should absorb *heat* far more largely than chlorine does.

(3) A differential thermometer charged with chlorine (and vitriol) was kept in a water-bath and exposed to direct sunlight. By alternately shading the one and the other of the bulbs I produced displacements of the index amounting to several centimetres, which I am inclined to ascribe essentially to the action of the chemical rays, because

(4) A CO^2 thermometer, under the same circumstances, exhibited no action ; and

(5) On shading the bulbs with a plate of blue cobalt-glass, about one quarter of the effect of the insolation remained.

The mere fact that there is a body which shows the phenomena in question is of great interest, suggesting, as it does, the possibility of constructing an " actinometer " which could be read off as easily as an ordinary mercury thermometer. I purpose undertaking experiments in this direction. It is not very likely that chlorine (and bromine) should be the only substance to show that remarkable behaviour in actinic rays.

As to the theoretical interpretation of the experiments, it seems to me that three views chiefly are worthy of being taken into consideration, viz. :—

(1) The assumption which suggested the investigation, that light actually decomposes chlorine molecules into chlorine atoms.

(2) One might assume that highly refrangible light, in acting upon chlorine, was doing a peculiar unknown kind of work, which in its turn was changed into heat and thus caused the expansion.

(3) One might say that the distinction made since the time of Seebeck and Melloni, between heating and non-heating but chemically active rays, had no sufficient foundation in fact, and look upon the phenomenon above detailed as a direct proof of the existence of bodies which are heated more strongly by violet than by red light. It is true, indeed, that that distinction is founded almost entirely upon the behaviour of rays towards a thermopile covered with lampblack, and therefore, strictly speaking, applies solely to lampblack as light-absorber. If, for instance, red light did *not* happen to heat this particular substance, we should perhaps not know those rays to be thermically active.

Of the above assumptions, the second appears to me the least plausible; while the first, on the other hand, is no little supported by the coincidence of the rays which make chlorine expand with those which are known to render it chemically active.

I have projected additional experiments for the further elucidation of the subject; but as I have no electric lamp, and hence the possibility of their being carried out depends entirely on the state of the weather, I considered it best, in the mean time, to publish my present observations as they are.

Bromine, according to a few preliminary experiments, behaves like chlorine; other substances than these two have not as yet been tried.

Bonn, September 1, 1871.

XXXII. *On a Class of Definite Integrals.*
By J. W. L. Glaisher, *B.A., F.R.A.S., F.C.P.S.**

THE Theory of Definite Integrals, strictly speaking, is confined within a very small compass; in fact it can scarcely be said that there exists a Theory of Definite Integrals in the same sense as we speak of the Theory of Equations, the Theory of Curves, &c.: the integrals are evaluated, but their properties are not, as a rule, studied. The majority of works having the

title are devoted to the evaluation of Integrals by different iso-
lated methods, which, though well adapted for the purpose and
interesting, are not connected together as parts of a theory. A
similar want of system holds with regard to the integrals them-
selves. Many have been evaluated on account of their use in
physics, a greater number on account of their intrinsic interest
or elegance, more still as examples of different and frequently
highly ingenious modes of evaluation; while in not a few cases
it is hard to see what inducement tempted their authors to
spend time over them. The subject of Definite Integrals, re-
garded as a science, is still rather in the observational than the
theoretical state; *i. e.* the results have more resemblance to
detached facts observed than to a chain of facts connected by a
theory; and so it must for a long time remain.

Any one who takes up a memoir or work on the subject (such
as Meyer's *Theorie der bestimmten Integrale,* published during
the present year) must at once notice that, except in a few cases
such as the Elliptic Functions, the Gamma Function, &c., which
are usually considered separately, a number of definite integrals
are proved equal to certain quantities, without any indication
being apparent why those given should have been preferred to
others omitted, or, in the absence of any properties of the func-
tions, for what purpose they were evaluated. The fact seems to
be that every definite integral, not so complicated or unsymme-
trical in form as to be absolutely destitute of interest, which
admits of finite expression, or of expression in a tolerable simple
series, has been evaluated; and the gaps which occur are due to
the integrals omitted not being so expressible. Thus, for ex-
ample, $\int_0^\infty \dfrac{\cos bx\, dx}{a^2 + x^2} = \dfrac{\pi}{2a}\, e^{-ab}$; but $\int_0^\infty \dfrac{\sin bx\, dx}{a^2 + x^2}$ is not expres-
sible in terms of ordinary functions. The number of what may be
called principal integrals evaluable is not large; and that this
is the case is not remarkable, when it is considered that there is
scarcely a function which cannot be thrown into the form of a
definite integral, while for the evaluation of the latter we can
only employ combinations of algebraical, circular, logarithmic,
and exponential quantities. For the advance of the subject,
therefore, the introduction of new fundamental functions is a
necessity; and Schlömilch, in order to evaluate the second of the
integrals written above and some few others of allied form, made
use of the functions known as the sine-integral, cosine-integral,
and exponential-integral. As soon as it appeared that these were
suitable primary functions, a large number of definite integrals,
previously inexpressible, were reduced to dependence on them,
their properties were investigated, and Tables constructed of
their numerical values.

This procedure was truly scientific, and has extended the limits of the science ; and a similar course must continue to be pursued, not only with the view of increasing the number of integrals which, if need be, could be calculated numerically, but also for the sake of making the subject more systematic and homogeneous in form, as well as connecting the different results with more completeness and unity.

The fact also, previously alluded to, of the power of definite integrals as a means of expressing other functions (such as solutions of algebraical and differential equations &c.), points to the value of a good classification accompanied by full numerical Tables.

The chief point of importance, therefore, is the choice of the elementary functions ; and this is a work of some difficulty. One function, however, viz. the integral $\int_x^\infty e^{-x^2} dx$, well known for its use in physics, is so obviously suitable for the purpose, that, with the exception of receiving a name and a fixed notation, it may almost be said to have already become primary. I propose, therefore, in the present communication to investigate some of the most important integrals evaluable by its means, and several connected results—and in a subsequent communication, after noticing a few of the principal physical results involving it, to describe the Tables that have been calculated of its numerical values, and supplement them by a Table with different arguments, which is nearly completed.

As it is necessary that the function should have a name, and as I do not know that any has been suggested, I propose to call it the *Error-function*, on account of its earliest and still most important use being in connexion with the theory of Probability, and notably the theory of Errors, and to write

$$\int_x^\infty e^{-x^2} dx = \text{Erf } x. \quad . \quad . \quad . \quad . \quad (1)$$

We then have the following results obtained by obvious transformations :

$$\int_x^\infty e^{-a^2 x^2} dx = \frac{1}{a} \text{Erf } ax, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\int_x^\infty e^{-ax} \frac{dx}{\sqrt{x}} = \frac{2}{\sqrt{a}} \text{Erf } \sqrt{ax}, \quad . \quad . \quad . \quad . \quad (3)$$

$$\int_x^\infty e^{-a(x+b)^2} dx = \frac{1}{\sqrt{a}} \text{Erf } (x+b) \sqrt{a} ; \quad . \quad . \quad (4)$$

also $\mathrm{Erf}\, 0 = \frac{1}{2}\sqrt{\pi}$, so that

$$\int_0^x e^{-x^2}dx = \frac{1}{2}\sqrt{\pi} - \mathrm{Erf}\, x,$$

$$\int_0^x e^{-ax}\frac{dx}{\sqrt{x}} = \sqrt{\frac{\pi}{a}} - \frac{2}{\sqrt{a}}\mathrm{Erf}\sqrt{ax}, \&c.$$

We know that

$$\int_0^\infty e^{-cx^2}dx = \frac{\sqrt{\pi}}{2\sqrt{c}},$$

whence

$$\int_0^\infty e^{-c(x^2+a^2)}dx = \frac{\sqrt{\pi}}{2}\frac{e^{-a^2c}}{\sqrt{c}};$$

whence, integrating with regard to c,

$$\int_0^\infty \frac{e^{-c(x^2+a^2)}}{x^2+a^2}dx = \frac{\sqrt{\pi}}{a}\mathrm{Erf}\, a\sqrt{c}$$

from (3), and therefore

$$\int_0^\infty \frac{e^{-cx^2}}{x^2+a^2}dx = \frac{\sqrt{\pi}}{a}e^{a^2c}\mathrm{Erf}\, a\sqrt{c}. \quad . \quad . \quad . \quad (5)$$

This result is not new; it is obtained, though in a different manner, in De Morgan's 'Diff. and Int. Calc.' p. 676.

From it we can deduce, by Boole's theorem

$$\int_{-\infty}^\infty \phi\left(x - \frac{\alpha}{x}\right)dx = \int_{-\infty}^\infty \phi(x)\, dx,$$

α being positive (Phil. Trans. 1857, p. 780), that

$$\int_0^\infty \frac{x^2 e^{-c\left(x^2+\frac{\alpha^2}{x^2}\right)}dx}{x^4+(a^2-2\alpha)x^2+\alpha^2} = \frac{\sqrt{\pi}}{a}e^{a^2c-2\alpha c}\mathrm{Erf}\, a\sqrt{c}, \quad . \quad (6)$$

or, taking $x = \frac{1}{z}$, changing the values of the constants, and writing x for z,

$$\int_0^\infty \frac{e^{-c\left(x^2+\frac{\alpha^2}{x^2}\right)}dx}{x^4+(a^2-2\alpha)x^2+\alpha^2} = \frac{\sqrt{\pi}}{a\alpha}e^{a^2c-2\alpha c}\mathrm{Erf}\, a\sqrt{c}. \quad . \quad (7)$$

Putting $\alpha = \frac{a^2}{2}$, we have, as particular cases,

$$\int_0^\infty \frac{x^2 e^{-c\left(x^2+\frac{\alpha^2}{x^2}\right)}}{x^4+\alpha^2}dx = \sqrt{\frac{\pi}{2\alpha}}\mathrm{Erf}\,\sqrt{2\alpha c}, \quad . \quad (8)$$

$$\int_0^\infty \frac{e^{-c\left(x^2+\frac{\alpha^2}{x^2}\right)}}{x^4+\alpha^2}dx = \sqrt{\frac{\pi}{2\alpha^3}}\mathrm{Erf}\,\sqrt{2\alpha c}. \quad . \quad . \quad (9)$$

The former result can be verified by integrating both sides of the equation

$$\int_0^\infty e^{-c\left(x^2+\frac{\alpha^2}{x^2}\right)}dx = \frac{\sqrt{\pi}}{2}\frac{e^{-2\alpha c}}{\sqrt{c}}$$

with respect to c.

The integral $\int_0^\infty e^{-(ax^2+bx+c)}\,dx$ is simply expressible in terms of the error-function; for the former

$$= e^{-c+\frac{b^2}{4a}}\int_0^\infty e^{-a\left(x+\frac{b}{2a}\right)^2}dx = \frac{1}{\sqrt{a}}\,e^{\frac{b^2-4ac}{4a}}\,\mathrm{Erf}\,\frac{b}{2\sqrt{a}}. \quad . \quad (10)$$

Integrate both sides of the well-known equation

$$\int_0^\infty e^{-a^2x^2}\cos 2rx\,dx = \frac{\sqrt{\pi}}{2a}\,e^{-\frac{r^2}{a^2}}. \quad . \quad . \quad . \quad (11)$$

with regard to r between the limits r and 0, and we have

$$\int_0^\infty e^{-a^2x^2}\frac{\sin 2rx}{2x}\,dx = \frac{\sqrt{\pi}}{2a}\cdot\left\{a\frac{\sqrt{\pi}}{2}-a\,\mathrm{Erf}\,\frac{r}{a}\right\};$$

that is,

$$\int_0^\infty e^{-a^2x^2}\sin 2rx\,\frac{dx}{x} = \frac{\pi}{2}-\sqrt{\pi}\,\mathrm{Erf}\frac{r}{a}. \quad . \quad . \quad . \quad (12)$$

By differentiating (11) with regard to r and dividing by r, we obtain

$$\int_0^\infty e^{-a^2x^2}\frac{\sin 2rx}{r}\cdot x\,dx = \frac{\sqrt{\pi}}{2a^3}\,e^{-\frac{r^2}{a^2}}.$$

From this we deduce, by integrating with regard to r,

$$\int_0^\infty xe^{-a^2x^2}\,\mathrm{Si}(2rx)dx = \frac{\pi}{4a^2}-\frac{\sqrt{\pi}}{2a^2}\,\mathrm{Erf}\frac{r}{a}. \quad . \quad (13)$$

De Morgan (Diff. and Int. Calc. p. 675) obtained a formula which, when slightly altered in form and generalized, may be written

$$\int_0^\infty \frac{e^{-cx^2}\cos 2bx}{a^2+x^2}\,dx$$
$$= \frac{\sqrt{\pi}}{2a}e^{a^2c}\left\{e^{-2ab}\,\mathrm{Erf}\left(a\sqrt{c}-\frac{b}{\sqrt{c}}\right)+e^{2ab}\,\mathrm{Erf}\left(a\sqrt{c}+\frac{b}{\sqrt{c}}\right)\right\}; \quad (14)$$

and differentiating with regard to b, we have

$$\int_0^\infty \frac{e^{-cx_2}\sin 2bx}{a^2+x^2}\,xdx$$
$$= \frac{\sqrt{\pi}}{2}e^{a^2c}\left\{e^{-2ab}\,\mathrm{Erf}\left(a\sqrt{c}-\frac{b}{\sqrt{c}}\right)-e^{2ab}\,\mathrm{Erf}\left(a\sqrt{c}+\frac{b}{\sqrt{c}}\right)\right\}. \quad (15)$$

Among integrals of less interest may be noticed

$$\int_0^\infty (\cos px - \sin px) \log\left(1 + \frac{q^2}{x^2}\right) \frac{dx}{\sqrt{x}}$$
$$= \frac{\pi}{\sqrt{2p}} \left\{ \sqrt{\pi} - 2 \operatorname{Erf} \sqrt{pq} \right\}, \quad . \quad (16)$$

and

$$\int_0^{\frac{\pi}{2}} e^{-q^2(\tan x + \cot x)} \sqrt{\tan x} \, dx = \sqrt{2\pi} \operatorname{Erf} q \sqrt{2}, \quad . \quad (17)$$

obtained from results given in De Haan's *Nouvelles Tables,* No. 11, Table 178, and No. 9, Table 276.

There are several simple formulæ involving the function under the integral sign. Thus from

$$\int_0^\infty \frac{e^{-ax}}{\sqrt{a}} dx = \frac{1}{a^{\frac{3}{2}}}$$

we deduce

$$\int_0^\infty \operatorname{Erf} \sqrt{ax} \frac{dx}{\sqrt{x}} = \frac{1}{\sqrt{a}}; \quad . \quad . \quad . \quad . \quad (18)$$

from

$$\int_0^\infty e^{-a^2 x^2} dx = \frac{\sqrt{\pi}}{2a},$$

by integrating between limits, we find

$$\int_0^\infty (\operatorname{Erf} ax - \operatorname{Erf} bx) \frac{dx}{x} = \frac{\sqrt{\pi}}{2} \log\frac{b}{a}; \quad . \quad . \quad (19)$$

and from

$$\int_0^\infty e^{-c^2 x^2 - \frac{a^2}{x^2}} dx = \frac{\sqrt{\pi}}{2c} e^{-2ac}$$

we deduce

$$\int_0^\infty x e^{-c^2 x^2} \operatorname{Erf} \frac{a}{x} dx = \frac{\sqrt{\pi}}{4c^2} e^{-2ac}. \quad . \quad . \quad . \quad (20)$$

By the aid of Fourier's theorem, that if

$$f(r) = \sqrt{\frac{2}{\pi}} \int_0^\infty \phi(x) \sin rx \, dx,$$

then

$$\phi(x) = \sqrt{\frac{2}{\pi}} \int_0^\infty f(r) \sin rx \, dr,$$

we derive from (12), which may be written

$$\sqrt{\frac{2}{\pi}} \int_0^\infty \frac{e^{-\frac{x^2}{4a^2}}}{x} \sin rx \, dx = \sqrt{\frac{\pi}{2}} - \sqrt{2} \operatorname{Erf} ar,$$

the result

$$\int_0^\infty \sin rx\, dr - \frac{2}{\sqrt{\pi}} \int_0^\infty \text{Erf } ar \sin rx\, dr = \frac{e^{-\frac{x^2}{4a^2}}}{x},$$

in which it is easy to see that we may legitimately put $\cos \infty = 0$ and obtain

$$\int_0^\infty \text{Erf } ar \sin rx\, dr = \frac{\sqrt{\pi}}{2x}\left(1 - e^{-\frac{x^2}{4a^2}}\right). \quad . \quad . \quad (21)$$

This result can also be obtained independently and in a more simple manner by integrating

$$\int_0^\infty e^{-a^2x^2} \sin rx\, x\, dx = \frac{r\sqrt{\pi}}{4a^3} e^{-\frac{r^2}{4a^2}}$$

with regard to a.

The well-known formula

$$\int_0^\infty e^{-a^2x} \cos bx\, dx = \frac{a^2}{a^4 + b^2}$$

affords, on integration,

$$\int_0^\infty \text{Erf } a\sqrt{x} \cos bx\, \frac{dx}{\sqrt{x}} = \int_a^\infty \frac{a^2}{a^4 + b^2}$$

$$= \frac{1}{2\sqrt{2b}}\left\{\pi - \frac{1}{2}\log\frac{a^2 - a\sqrt{2b} + b}{a^2 + a\sqrt{2b} + b} - \tan^{-1}\left(a\sqrt{\frac{2}{b}} + 1\right)\right.$$

$$\left. - \tan^{-1}\left(a\sqrt{\frac{2}{b}} - 1\right); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)\right.$$

and similarly from

$$\int_0^\infty e^{-a^2x} \sin bx\, dx = \frac{b}{a^4 + b^2}$$

we deduce

$$\int_0^\infty \text{Erf } a\sqrt{x} \sin bx\, \frac{dx}{\sqrt{x}}$$

$$= \frac{1}{2\sqrt{2b}}\left\{\frac{1}{2}\log\frac{a^2 - a\sqrt{2b} + b}{a^2 + a\sqrt{2b} + b} + \tan^{-1}\left(\frac{\sqrt{2b}}{a} + 1\right)\right.$$

$$\left. + \tan^{-1}\left(\frac{\sqrt{2b}}{a} - 1\right)\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

Many other formulæ could, no doubt, be found; but the above probably include the most simple cases. When the constants have imaginary values assigned to them, the results sug-

gest the real values of the integrals; for example, write $bi\ (i = \sqrt{-1}$ as usual) for b in

$$\int_0^\infty e^{-ax^2 - 2bx}\, dx = \frac{1}{\sqrt{a}} e^{\frac{b^2}{a}} \operatorname{Erf} \frac{b}{\sqrt{a}},$$

and there results

$$\int_0^\infty e^{-ax^2}(\cos 2bx - i \sin 2bx)\, dx = \frac{1}{\sqrt{a}} e^{-\frac{b^2}{a}} \operatorname{Erf} \frac{bi}{\sqrt{a}},$$

and

$$\operatorname{Erf} \frac{bi}{\sqrt{a}} = \frac{\sqrt{\pi}}{2} - \int_0^{\frac{bi}{\sqrt{a}}} e^{-x^2}\, dx = \frac{\sqrt{\pi}}{2} - i \int_0^{\frac{b}{\sqrt{a}}} e^{x^2}\, dx,$$

whence

$$\int_0^\infty e^{-ax^2} \cos 2bx\, dx = \frac{\sqrt{\pi}}{2\sqrt{a}} e^{-\frac{b^2}{a}},$$

and

$$\int_0^\infty e^{-ax^2} \sin 2bx\, dx = \frac{1}{\sqrt{a}} e^{-\frac{b^2}{a}} \int_0^{\frac{b}{\sqrt{a}}} e^{x^2}\, dx.$$

The former result is well known; and the latter is easily verified by differentiating with respect to b and forming a differential equation, from which the value of the integral can be determined.

As another example, writing bi for b in (14), and noticing that

$$\int_{-\infty}^\infty e^{-cx^2} \sin 2bx\, dx = 0,$$

we find

$$\int_{-\infty}^\infty \frac{e^{-cx^2 \pm 2bx}}{a^2 + x^2}\, dx$$
$$= \frac{\sqrt{\pi}}{a} e^{a^2 c} \left\{ e^{2abi} \operatorname{Erf}\left(a\sqrt{c} + \frac{bi}{\sqrt{c}}\right) + e^{-2abi} \operatorname{Erf}\left(a\sqrt{c} - \frac{bi}{\sqrt{c}}\right) \right\}. \quad (24)$$

Now $\operatorname{Erf}\left(a\sqrt{c} + \frac{bi}{\sqrt{c}}\right) - \operatorname{Erf}(a\sqrt{c}) = -\int_{a\sqrt{c}}^{a\sqrt{c} + \frac{bi}{\sqrt{c}}} e^{-x^2}\, dx$

$$= -\frac{e^{-a^2c}}{\sqrt{c}} \int_0^{bi} e^{-\frac{x^2}{c} - 2ax}\, dx = \frac{e^{-a^2c}}{i\sqrt{c}} \int_0^b e^{\frac{b^2}{c} - 2abi}\, db,$$

whence

$$\operatorname{Erf}\left(a\sqrt{c} + \frac{bi}{\sqrt{c}}\right) + \operatorname{Erf}\left(a\sqrt{c} - \frac{bi}{\sqrt{c}}\right) = 2 \operatorname{Erf} a\sqrt{c}$$
$$- 2\frac{e^{-2a^2c}}{\sqrt{c}} \int_0^b e^{\frac{b^2}{c}} \sin 2ab\, db,$$

$$\operatorname{Erf}\left(a\sqrt{c} + \frac{bi}{\sqrt{c}}\right) - \operatorname{Erf}\left(a\sqrt{c} - \frac{bi}{\sqrt{c}}\right) = 2\frac{e^{-a^2c}}{i\sqrt{c}} \int_0^b e^{\frac{b^2}{c}} \cos 2ab\, db,$$

and we have

$$\int_{-\infty}^{\infty} \frac{e^{-cx^2 \pm 2bx}}{a^2 + x^2}\, dx$$

$$= 2\,\frac{\sqrt{\pi}}{a} \left\{ e^{a^2 c}\, \mathrm{Erf}\,(a\sqrt{c})\, \cos 2ab + \frac{\sin 2ab}{\sqrt{c}} \int_0^b e^{\frac{b^2}{c}} \cos 2ab\, db \right.$$
$$\left. - \frac{\cos 2ab}{\sqrt{c}} \int_0^b e^{\frac{b^2}{c}} \sin 2ab\, db \right\}, \quad . \quad (25)$$

a result which can be verified by forming the differential equation

$$\frac{d^2 y}{db^2} + 4a^2 y = 4 \int_{-\infty}^{\infty} e^{-cx^2 \pm 2bx}\, dx = 4 e^{\frac{b^2}{c}} \sqrt{\frac{\pi}{c}},$$

y denoting the integral, which, on integrating and determining the constants by the considerations that the result must be independent of the sign of b, and that when $b = 0$ it must equal

$$\frac{2\,\sqrt{\pi}\, e^{a^2 c}}{a}\, \mathrm{Erf}\,(a\sqrt{c}),$$

gives the same expression for y.

The result (25) may be written in another form for

$$\mathrm{Erf}\left(a\sqrt{c} + \frac{bi}{\sqrt{c}}\right) = \int_{a\sqrt{c}}^{\infty} e^{-\left(x + \frac{bi}{\sqrt{c}}\right)^2} dx$$

$$= e^{\frac{b^2}{c}} \int_{a\sqrt{c}}^{\infty} e^{-x^2} \left(\cos \frac{2bx}{\sqrt{c}} - i \sin \frac{2bx}{\sqrt{c}} \right) dx,$$

so that

$$\mathrm{Erf}\left(a\sqrt{c} + \frac{bi}{\sqrt{c}}\right) + \mathrm{Erf}\left(a\sqrt{c} - \frac{bi}{\sqrt{c}}\right) = 2 e^{\frac{b^2}{c}} \int_{a\sqrt{c}}^{\infty} e^{-x^2} \cos \frac{2bx}{\sqrt{c}}\, dx,$$

$$\mathrm{Erf}\left(a\sqrt{c} - \frac{bi}{\sqrt{c}}\right) - \mathrm{Erf}\left(a\sqrt{c} + \frac{bi}{\sqrt{c}}\right) = 2 i e^{\frac{b^2}{c}} \int_{a\sqrt{c}}^{\infty} e^{-x^2} \sin \frac{2bx}{\sqrt{c}}\, dx.$$

XXXIII. *On a new Method of solving some Problems in the Calculus of Variations, in reply to* Professor Cayley. *By* the Rev. Professor Challis, *M.A., F.R.S.*[*]

IN an article in the Number of the Philosophical Magazine for September, Professor Cayley has expressed his dissent from the new method of solving certain problems in the Calculus of Variations which is contained in my communication to the Number for July. On carefully considering all that he has

said, I find that no argument is adduced which does not rest on the assumption that the equations $Ap = 0$ and $A = 0$ are *necessarily identical*. To this assumption, which is made without the support of any reasoning, I oppose the general argument, that as the two equations differ symbolically (the former being of a degree superior by one to that of the other), they cannot be *necessarily* equivalent, inasmuch as in that case the symbolic difference between them would have no signification, which is contradictory to the principles of analysis. This *à priori* reason I proceed to confirm by the following particular considerations.

There are instances in which $Ap = 0$ and $A = 0$ are both integrable *per se*, and the two integrals are identical. The Problem I., solved in my article in the July Number, presents one such instance. In other cases, as that of the problem proposed by Mr. Todhunter in p. 410 of his ' History of the Calculus of Variations,' only $Ap = 0$ is integrable *per se*, but the integral satisfies $A = 0$. In these two classes of solutions the Calculus gives for each problem a unique result.

There are also instances in which a solution is effected by an integral of $Ap = 0$ which does *not* satisfy $A = 0$. This is the case with respect to the problem of the greatest solid of revolution of given superficial area, the surface being subject to the condition of passing through two given points of the axis. The discontinuous solution of this problem obtained by Mr. Airy in the Number of the Philosophical Magazine for July 1861, is deduced exclusively from the equation $Ap = 0$. In the Number for June 1866 Mr. Todhunter has completed this solution by proving that it actually gives a maximum; but in so doing *he has expressly excluded the equation* $A = 0$. In short, in this instance the equation $Ap = 0$ is treated as if it were independent of the equation $A = 0$. I am entitled, I believe, to say that Professor Cayley assents, as I do, to the solution in question. Why, then, does he object to treating $A = 0$ independently of $Ap = 0$? The result which he accepts *proves* that the two equations are not necessarily equivalent.

In the July Number I have deduced solutions of Problems II. and III. by an independent treatment of the equation $A = 0$, and have thus obtained, in the case of Problem III., the continuous solution of the problem of which, as stated above, the Astronomer Royal gave a discontinuous solution. The novelty of the process I have adopted consists in deriving from the equation $A = 0$, regarded as the differential equation of a curve, the equation of the corresponding evolute, and then employing one of the involutes to satisfy the conditions of the proposed problem. In the case of Problem II. the equation of the evolute was explicitly obtained, and the appropriate involute could consequently

be immediately determined. With reference to this point, Professor Cayley says, "after the evolute is obtained, we must take not *any* involute, but the proper involute of such evolute." But, as Professor Cayley well knows, there is no proper involute of any evolute, because the number of the involutes of a given evolute is unlimited. What he calls "the proper involute" is the curve given by integrating the equation $Ap=0$; and on the before-mentioned gratuitous assumption that this equation and $A=0$ are identical, he refuses to recognize any involute derivable from the latter equation other than that curve. Of course I do not admit that this is an argument, because, for the reasons already urged, I maintain that there is no ground for the initial assumption.

The equation of the involute which gives the solution of Problem II. contains *three* arbitrary constants, because it involves the arbitrary length of the cord which, by unwinding from the evolute, describes that involute. By eliminating the three constants a differential equation of the third order is obtained, which is evidently not identical with the equation $A=0$ of the second order. Neither is it identical with $\dfrac{dA}{dx}=0$, because it cannot be satisfied if $A =$ a constant. But it is found that that equation of the third order is verified by substituting for $\dfrac{d^2y}{dx^2}$ and $\dfrac{d^3y}{dx^3}$ the values of these differential coefficients deduced from $A = 0$ and its derived equation $\dfrac{dA}{dx}=0$. This is proof that the involute which may be regarded as the solution of the problem is strictly derived from, and exclusively depends upon, the equation $A=0$. The process of derivation by the intervention of an evolute I have called "a new integration," as being distinct from the mode of solution by ordinary integration.

The mathematical reasoning with which Professor Cayley concludes his communication only amounts to a proof that the integral of $Ap=0$ gives a curve which is included among the involutes of the evolute which was derived from the equation $A=0$. I have no remark to make on this result, as I had already obtained the same in the article in the July Number.

For the reasons above alleged, I adhere to the statement made at the end of the former communication, namely that I have succeeded in removing from analytics the reproach of failing to solve certain problems in the Calculus of Variations.

Cambridge, September 6, 1871.

XXXIV. *Contributions to the History of the Phosphorus Chlorides. By* T. E. THORPE, *Ph.D., F.R.S.E.*[*]

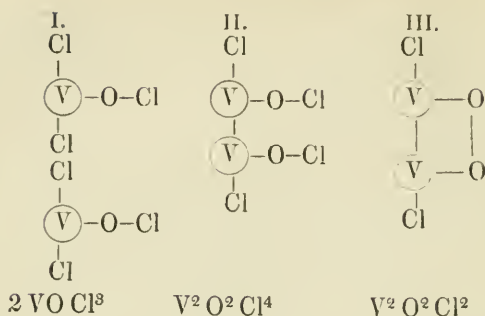### I. *On the Reduction of Phosphoryl Trichloride.*

IN his first memoir on Vanadium, Dr. Roscoe described a series of oxychlorides obtained from vanadyl trichloride by the action of reducing-agents. When the vapour of vanadyl trichloride is passed together with hydrogen through a heated tube, a bright grass-green crystalline sublimate of vanadyl dichloride, $VO Cl^2$, is produced in the anterior portion of the tube; afterwards a layer of vanadyl monochloride, $VO Cl$, is deposited as an exceedingly light, flocculent, brown powder; whilst at the extreme end of the tube beautiful bronze-coloured plates of the divanadyl monochloride, $V^2 O^2 Cl$, are formed, which have the appearance of mosaic gold. In this memoir Dr. Roscoe clearly pointed out the intimate analogy which exists between the compounds of vanadium and those of phosphorus, arsenic, antimony, and nitrogen; and in his subsequent researches on this subject, he has so far elaborated this view of its chemical relationship, that there is no longer room to doubt that vanadium is virtually a member of the trivalent group of elements.

It must be confessed, however, that the triatomic nature of vanadium is not very apparent in the oxychlorides derived from the vanadyl trichloride if the simplest formulæ derived from their analysis are retained; but if these formulæ be doubled, the difficulty at once vanishes. The supposition that these oxychlorides possess a greater molecular weight than the vanadyl trichloride, may derive some support from the fact of the change of physical state which accompanies their formation, the lower oxychlorides being all solid. Beyond this I am not aware that any fact is known to establish such an assumption, unless it be the coincidence between the atomic volume of the vanadyl trichloride and that of the vanadyl dichloride with the formula doubled.
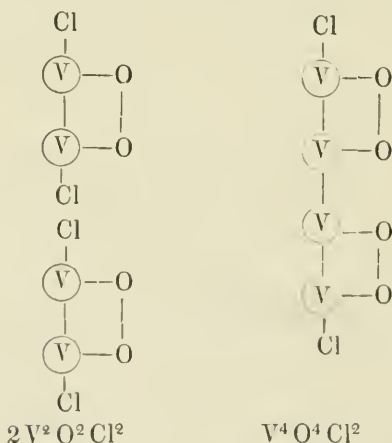
	Specific gravity.	Atomic weight.	Atomic volume.
$V O Cl^3$	1·83	173·8	95·0
$V^2 O^2 Cl^4$	2·88	276·6	96·0

According to this view, the formulæ of these oxychlorides and their relation to the vanadyl trichloride would be graphically represented thus:—

I.

Cl
|
(V)—O—Cl
|
Cl
Cl
|
(V)—O—Cl
|
Cl

II.

Cl
|
(V)—O—Cl
|
(V)—O—Cl
|
Cl

III.

Cl
|
(V) —O
| |
V   —O
|
Cl

$2 \, VO \, Cl^3$ $V^2 O^2 Cl^4$ $V^2 O^2 Cl^2$

And just as the $V^2 O^2 Cl^4$ is formed by the juxtaposition of two molecules of $VO Cl^3$ minus two atoms of chlorine, so in like manner the $V^4 O^4 Cl^2$ may be represented as derived from two molecules of $V^2 O^2 Cl^2$ minus two atoms of chlorine : thus,

Cl
|
(V)—O
| |
(V)—O
|
Cl
Cl
|
(V)—O
| |
(V)—O
|
Cl

$2 \, V^2 O^2 Cl^2$

Cl
|
(V)—O
| |
V —O
|
(V)—O
| |
V —O
|
Cl

$V^4 O^4 Cl^2$

So far as I am aware, there is nothing to disprove such a method of representation ; it has at least the merit of preserving the triatomic nature of vanadium in these compounds, and shows in a simple manner their relation to the vanadyl trichloride.

Assuming, then, that the triatomic nature of vanadium is established, analogy points to the existence of other oxychlorides among the trivalent group than those at present known to us. To fulfil the relationship, we ought to have $TO Cl^3$, $T^2 O^2 Cl^4$, $T^2 O^2$, Cl^2 and $T^4 O^4 Cl^2$, where T represents a member of the triatomic group of elements. The following Table represents these analogies so far as they are complete :—

$V\,O\,Cl^3$	——	$PO\,Cl^3$	——	$Sb\,SCl^3$
$V^2 O^2 Cl^4$	$N^2 O^2 Cl^4$	——	——	
$V^2 O^2 Cl^2$	$N^2 O^2 Cl^2$	——	$As^2 O^2 Cl^2*$	$Sb^2 O^2 Cl^2†$
$V^4 O^4 Cl^2$	——	——	——	

The $V^2 O^2 Cl^4$ was also prepared by Roscoe by heating $VO\,Cl^3$ in a sealed tube with fragments of metallic zinc to a temperature above the boiling-point of mercury; in this way the compound was obtained in quantity; and it was easily freed from a small quantity of adhering $VO\,Cl^3$ by heating to 130° C. in a current of dry carbon dioxide. I have attempted to repeat this reaction with phosphoryl trichloride. A quantity of the pure liquid was sealed up together with zinc filings in a tube and heated to about 400°. The zinc was slowly acted upon, and a transparent glassy mass was formed at the bottom of the tube. No evolution of gas occurred on opening the tube. The small quantity of liquid remaining was submitted to distillation; it commenced to boil at about 80°, and the thermometer gradually rose to 105°, by which time the whole of the liquid had passed over. This behaviour appeared to indicate the presence of phosphorus trichloride, PCl^3; and a few drops of the liquid decomposed by water yielded the reactions of phosphorous acid. The quantity was too small to admit of fractional distillation, even if the perfect separation of the two liquids had been practicable by this method.

Accordingly three portions of the distillate were weighed out for determination of the chlorine, total phosphorus, and amount of phosphorus yielding phosphoric acid on decomposition with water.

I. Determination of chlorine.—The weighed quantity of liquid was decomposed by water in a stoppered bottle, a quantity of nitric acid added, and the chlorine precipitated with silver nitrate.

0·6032 grm. gave 1·7764 AgCl and 0·0075 Ag.

Cl found.	Calc. for PO Cl³.	For PCl³.
73·21 per cent.	69·36	77·43

II. Determination of total phosphorus.—The mixed chlorides were decomposed by water in the manner above described, nitric acid added, and the liquid concentrated. The fluid was then made strongly alkaline by ammonia, and the phosphorus precipitated as the magnesium ammonium compound.

1·3343 grm. gave 1·0077 magnesium pyrophosphate, or 21·09 per cent. P.

III. Determination of the phosphorus existing as phosphoryl chloride, yielding phosphoric acid on decomposition with water.

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* Wallace's chlorarsenic acid.   † Schaeffer.

—The bulb containing the weighed portion was broken under water, and ammonia and "magnesia-mixture" immediately added.

1·3965 grm. gave 0·5174 magnesium pyrophosphate, or 10·58 per cent. P, equivalent to 52·35 per cent. PO Cl³.

These numbers almost exactly correspond to a mixture containing equivalent quantities of PO Cl³ and PCl³. Such a mixture would give :—

|								Found.
Cl	.	.	.	.	.	.	73·17	73·21
Total P.	.	.	.	.	.	21·32	21·09
P giving PO⁴ H³.	.	10·66	10·58

The vitreous mass remaining in the tube fused on gently heating, and was decomposed; it was probably a combination of one of the above chlorides with zinc chloride, possibly the $ZnCl^2 +$ PO Cl³, already described by Casselmann[*], mixed with zinc oxide or zinc oxychloride.

The action of zinc at a high temperature on phosphoryl trichloride is therefore sensibly different from the action of this metal on the corresponding vanadium compound : in the former case the reaction is mainly attended with the abstraction of oxygen, in the latter with the abstraction of chlorine.

II. *Note on the Preparation of Phosphorus Sulphochloride.*

This compound was first prepared by Serullas, who obtained it by the action of sulphuretted hydrogen upon the pentachloride of phosphorus,

$$PCl^5 + H^2 S = 2 HCl + P S Cl^3.$$
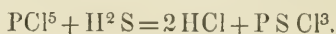
This reaction, however, affords only an impure product. Baudrimont states that this compound is more easily prepared by the action of pentachloride of phosphorus on antimony trisulphide,

$$3 PCl^5 + Sb^2 S^3 = 2 Sb Cl^3 + 3 P S Cl^3.$$
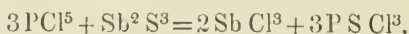
It has long been known that phosphoryl trichloride may be easily obtained in a state of complete purity by the action of pentachloride of phosphorus on phosphoric anhydride,

$$P^2 O^5 + 3 PCl^5 = 5 PO Cl^3.$$

It occurred to me to try whether the sulphochloride might not be produced by the analogous reaction with phosphorus pentasulphide,

$$P^2 S^5 + 3 PCl^5 = 5 P S Cl^3.$$

The materials mixed in this proportion were heated in sealed

* *Ann. der Chem. und Pharm.* vol. xcviii. p. 213.

tubes to about 150°; in a few minutes combination was quickly effected, and the entire contents of the tubes were transformed into colourless phosphorus sulphochloride, which boiled constantly at 126°·6 C. at 770 millims. barom. As thus obtained, it is a colourless mobile liquid; its vapour is extremely irritating, and possesses a sharp aromatic odour, which when diluted reminds one of that of the raspberry. It is but slowly decomposed by water.

XXXV. *On the Existence of Sulphur Dichloride.* By John Dalzell *and* T. E. Thorpe, *Ph.D., F.R.S.E.**

WHEN dry chlorine in excess is passed through molten sulphur, a dark red fuming liquid slowly distils over. This, on renewed distillation, commences to boil at about 50° or 60°, and the thermometer slowly rises to 136° or 137°, at which point it remains stationary, and the orange-yellow disulphide $Cl^2 S^2$ passes over. The fraction boiling below 136° frequently amounts to three fourths of the original quantity of liquid; on again submitting it to distillation the same order of things is repeated, and but a comparatively small portion distils over above 136°. At each distillation the liquid becomes lighter in colour, until at length, by long-continued boiling, it assumes the bright yellow of the disulphide, and boils constantly at 136–137°. This behaviour would seem to indicate the existence of some compound of chlorine and sulphur, which slowly undergoes decomposition on distillation, ultimately forming the disulphide; and the observations of Dumas and Soubeiran, and of Marchand, Davy, and Rose, point to a body richer in chlorine than the disulphide; and their analyses lead to the formula $S Cl^2$. On the other hand, Carius denies the existence of sulphur dichloride in the dark-red liquid obtained by heating sulphur in chlorine, and asserts that the compound analyzed by Dumas and others was a mixture in atomic proportions of the disulphide with a tetrachloride of sulphur hitherto unisolated $(Cl^2 S^2 + SCl^4 = 3 Cl^2 S)$. According to Carius, the amount of chlorine contained in the liquid, over and above that required by the formula $S^2 Cl^2$, is altogether dependent on the temperature. But the fact of the protracted distillation required to break up the product into a liquid boiling constantly at 136–137° implies that the excess of chlorine is held by some other force than that of mere solution; and at the same time we are not altogether without facts more directly indicating the existence of the dichloride. Rose has obtained

compound of this body with the terchloride of arsenic, $2AsCl^3.Cl^2S$; and according to Guthrie, it yields with the olefines compounds of the general formula $C^n H^{2n} Cl^2 S$. Hübner and Gueront have lately made an observation which tends also to support the idea of the existence of this body[*]. A quantity of the pure chloride, $S^2 Cl^2$, was placed in a strong freezing-mixture, and a current of dry chlorine passed through it for some time, the excess of this gas (that is, that existing merely in solution) being displaced by a stream of dry carbon dioxide passed through for three or four hours. Whilst still in the freezing-mixture, a small quantity of the chloride was withdrawn and analyzed, when numbers were obtained exactly agreeing with those required by the formula $Cl^2 S$.

We have repeated this experiment with precisely the same result. A quantity of pure $S^2 Cl^2$ was first prepared: it boiled constantly at 136–137°, and was analyzed with the following results:—

		Calculated.		Found. I.	Found. II.
S^2	. .	64	47·42	—	—
Cl^2	. .	71	52·58	52·48	52·67
		135	100·00		

About 20 grms. of this liquid was then saturated with chlorine, in the manner described by Hübner and Gueront; after the excess of chlorine was removed by carbon dioxide, it yielded the following numbers on analysis :—

		Calculated.		Found. I.	Found. II.	Dumas, Hübner, and Gueront.	
S	.	$=32$	31·07	—	—	31·9	30·5
Cl^2	.	$=71$	68·93	69·25	69·06	68·1	69·3
		103	100·00			100·0	99·8

The existence of this body would therefore appear to be fully proved; for it is scarcely possible that such an agreement can be the result of an accidental coincidence.

From these experiments, therefore, we draw the same conclusions as those deduced by Hübner and Gueront, viz. that there exist two compounds of chlorine and sulphur, analogous to the oxides of hydrogen—first, a non-volatile chloride having the formula SCl^2, corresponding to $H^2 O$, which on distillation splits up into chlorine and the second chloride, $S^2 Cl^2$, corresponding to peroxide of hydrogen,

$$2 Cl^2 S = S^2 Cl^2 + Cl^2.$$
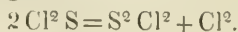
[*] *Zeitschrift für Chemie*, No. 15, 1870, p. 455.

It will be at once apparent that in one respect the analogy be-tween these chlorides and the oxides of hydrogen is incomplete, inasmuch as the most stable oxide of hydrogen is water, into which the dioxide is easily converted by heating—whereas the reverse of this happens with the corresponding sulphur chlorides, the most stable compound being the $S^2 Cl^2$, into which body and free chlorine the $S Cl^2$ is resolved on heating.

XXXVI. *On* Gauss's *Pentagramma mirificum.*
By Professor CAYLEY, *F.R.S.*[*]

TAKE on a sphere (in the northern hemisphere) two points, A, B, whose longitudes differ by 90°, and refer them to the equator by the meridians A E and B C respectively; join A, B by an arc of great circle, and take in the southern hemi-sphere the pole D of this circle; and join D with E and C re-spectively by arcs of great circle. We have a spherical pentagon A B C D E, which is in fact the "Pentagramma mirificum," con-sidered by Gauss, as appearing vol. iii. pp. 481–490 of the Col-lected Works.   Among its properties we have

the distance of any two non-adjacent summits $\left.\right\}=90°$;
the inclination of any two non-adjacent sides

so that each summit is the pole of the opposite side, or the pen-tagon is its own reciprocal.

Each angle is the supplement of the opposite side.

If the squared tangents of the sides (or angles) taken in order are α, β, γ, δ, ϵ, then

$$1+\alpha=\gamma\delta, \ \ 1+\beta=\delta\epsilon, \ \ 1+\gamma=\epsilon\alpha, \ \ 1+\delta=\alpha\beta, \ \ 1+\epsilon=\beta\gamma,$$

equivalent to three independent equations, so that any three of the quantities may be expressed in terms of the remaining two. (This agrees with the foregoing construction, where the arbitrary quantities are the latitudes of A, B respectively.)

Projecting from the centre of the sphere upon any plane, we have a plane pentagon which is such that the perpendiculars let fall from the summits upon the opposite sides respectively meet in a point.   This (as easily seen) implies that the two portions into which each perpendicular is divided by the point in question have the same product.

Conversely, starting from the plane pentagon, and erecting from the point of intersection a perpendicular to the plane, the length of this perpendicular being equal to the square root of the product in question, we have the centre of a sphere such that the projection upon it of the plane polygon is the penta-gramma mirificum.

I remark as to the analytical theory, that, taking the origin at the intersection of the perpendiculars, and for the coordinates of the summits $(\alpha_1, \beta_1), \ldots (\alpha_5, \beta_5)$ respectively, then we have

$$\alpha_1\alpha_4 + \beta_1\beta_4 = \alpha_2\alpha_5 + \beta_2\beta_5 = \alpha_3\alpha_1 + \beta_3\beta_1 = \alpha_4\alpha_2 + \beta_4\beta_2$$
$$= \alpha_5\alpha_3 + \beta_5\beta_3, = -\gamma^2,$$

where γ^2 is the above-mentioned product, or γ is the radius of the sphere.

Cambridge, September 14, 1871.

XXXVII. *Notices respecting New Books.*

Text-books of Science. Elements of Plane and Solid Geometry. By H. W. Watson, *M.A., some time Fellow of Trinity College, Cambridge.* Small 8vo, pp. 285. London: Longmans, 1871.

IT is scarcely possible to take an elementary treatise on geometry in hand without comparing it with Euclid's Elements; and we shall most readily convey to our readers a notion of the contents of this volume by stating that it covers very nearly the same ground as a good school edition of Euclid; that is to say, it contains, in substance, the first six books and the first twenty-one propositions of the eleventh book, with such additional propositions and remarks as are commonly given in the form of notes. On the other hand, the demonstrations of the propositions are in most cases not the same as Euclid's, and the order in which they are arranged is materially different. The work, in fact, is not in any sense an edition of Euclid, but a distinct and independent treatise on geometry, every page of which shows that a great deal of care and thought have been bestowed on its composition. It would not be easy in a short notice to mention the various points of difference, which in many cases extend to minute details; but the following may be particularized:— The propositions relating to areas, corresponding to Euclid I. 35–48 and II., form a separate book; the problems are kept distinct from the theorems, and form separate sections; the subject of proportion is treated algebraically; Loci are introduced at the end of the first book; in several parts Limits are employed; and all the books are subdivided into sections: *e. g.* the book on Planes and Lines in Space (a subject which is treated very elaborately) is subdivided into four sections, under the head of Miscellaneous Propositions, Perpendiculars and Obliques to Planes, Dihedral Angles, and Polyhedral Angles. Exercises, about 200 in all, are added to most of the sections.

The first section of the first book is on triangles; and the starting-point is an axiom equivalent to the familiar statement that "a straight line is the shortest distance between its extreme points." In Mr. Watson's hands, however, the axiom takes the following form:—"The length of the straight line joining any two points is less than the length of any broken line whatever joining the same two points"

(p. 5).   On turning to p. 93, the reason for adopting this unusual form of statement becomes apparent; and as the point is of some interest we will let Mr. Watson speak for himself:—

" When we speak of one straight line as being *equal* to another, or more accurately as being *equal in length* to another, we have a very exact and precise idea of what is meant, viz. that the one line can be applied to the other so that the extremities and every intermediate point of the one may coincide with the extremities and some intermediate point of the other.   So, again, when we speak of one circular arc as being *equal* to another circular arc of *equal radius*, we mean that the one arc can be applied to the other so that the extremities and every intermediate point of the one may coincide with the extremities and some intermediate point of the other.   But our ideas of equality of length are not confined to lines which may be superposed upon one another in this manner, and in practice we speak familiarly of the *lengths* of curves or of circular arcs, meaning thereby the lengths of straight lines equal in length to these curves or circular arcs, although a straight line and the arc of a circle cannot of course be superposed one upon the other.   It is well therefore to have an accurate definition of the length of a curved line, which we will now proceed to investigate."

The whole of Mr. Watson's remarks on this point will well repay perusal.   It might not occur to the reader that there is any difficulty about the conception of the length of a curved line.   He might say that we are made familiar, by our everyday experience, with threads that are flexible and sensibly inextensible, and that in consequence we have no more difficulty in forming the conception of a perfectly flexible and inextensible line, than in forming the conception of a line devoid of breadth or thickness.   Moreover we are familiar with such phenomena as the unwinding of cotton from a reel, or the paying out of a rope.   Such experiences, he might say, suggest the notion of a curved line pulled straight (or *rectified*), and that these conceptions could be easily embodied in a definition.   In the present work the question is thus treated : the nature of a rolling motion is first explained, and the result embodied in the following definition:—" If a straight line be made to roll upon a curved line, the length of the straight line between its first and last points of contact with the curved line is defined to be the length of the curved line between its first and last points of contact with the straight line."

The only objection to this way of looking at the matter is that the notion of a rolling motion is less familiar than that of a flexible line pulled straight.   What, however, is of most importance from the mathematician's point of view, is how the result of the recti- fication is to be determined; and this, after the necessary explana- tions, Mr. Watson states thus :—" If a number of points be taken between the first and last points on any finite curved line, and the chords between each pair of points in succession be drawn, and if the number of such points be indefinitely increased, and the length of each chord be indefinitely diminished, the *ultimate sum* of the lengths of these chords is the length of the curved line."   In this, we sup-

pose, all will agree ; but we do not quite see why Mr. Watson should call this statement a *definition*. If any distinction is to be made between definitions and axioms or postulates, surely the above statement is a sort of compound axiom or postulate consisting of at least two propositions : viz. (1) that the sum of the chords when their number is indefinitely increased will have a determinate finite limit ; (2) that this limit will be the length of the curve defined as above.

Although there are some other points on which we are not inclined to adopt Mr. Watson's views, we willingly allow that his views are in all cases well worth consideration, and that his book is an excellent treatise on the elements of geometry. When a second edition is called for, we would suggest that some of the diagrams should be drawn again. In a book of this kind obvious inaccuracy in the diagrams ought to be avoided, for the learner's sake ; and this has not always been done. *E. g.* p. 132, A F is obviously not a square ; p. 138, A B C D E is obviously less than P G ; p. 219, the pentagon is regular only by courtesy ; and so on in several other cases.

Select Methods in Chemical Analysis (*chiefly Inorganic*). *By* WIL-
LIAM CROOKES, *F.R.S. &c. Illustrated with* 22 *woodcuts.* Lon-
don : Longmans. 8vo, pp. xvi and 468.

It has been the intention of the author to provide the student with a laboratory companion containing information not usually found in ordinary works on analysis. The methods here placed at the student's disposal have all appeared during the last twelve years in the ' Chemical News,' of which Mr. Crookes is the well-known editor ; but in this work they are systematically arranged, and the compiler assures us that he has tested most and improved some of them (p. iii).

The " contents " are divided into chapters ; and in each chapter a group of the simple bodies is discussed, modes of separation from all the preceding bodies being then given. Attention is very properly paid to the rarer elements, whose presence is unjustly ignored in most treatises of this kind. But the work of systematization spoken of in the preface does not proceed very far; it is limited, as in most other manuals, to natural-history grouping. Even here the results are occasionally open to question. Does magnesium, for example, as closely resemble barium, strontium, or calcium as they resemble each other ? Yet all four metals are classed together in Chapter II. An entire chapter intervenes between glucinum and aluminium. Iron is divorced from manganese, silver from lead. It would probably be better to reject the natural-history method altogether, and follow the order of the atomic weights ; at any rate, we should then be proceeding on an intelligible principle, open to little dispute, and conducive to fresh comparison.

The " preparations " incidental to analytical work are noticed here and there. Thus very good methods are given for making pure sulphuric acid, pure lead, zinc, &c. ; on the other hand, the way of making phosphorous acid (p. 337) leads to no useful result. Most

of the " processes " promise well, and appear to have been selected with considerable care ; not a few of them have been repeatedly ve- rified by the constant use of chemists. Terreil's inaccurate method for determining cobalt (p. 151), however, is a decided blemish. Chapter XIV., on gas-analysis, is too short and deficient by leaving out of mention many of the great improvements in the measurement of gases that have been introduced of late years. To the few pages devoted to organic analysis, an account of Ladenburg's method ought certainly to have been annexed. We may add, also, that the author does not, as a rule, by any means sufficiently mark the nature and extent of his own contributions ; and he has given to American methods a prominence which is peculiar and very noticeable.

Mr. Crookes's book is not without its defects ; but it is the first attempt of its kind, and on that score alone would deserve much allowance. But the extensive verification to which he has subjected his material renders it much more valuable and meritorious. The possession of such a treatise cannot fail to economize the time of the general, but especially of the technological analyst.

XXXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 232.]

April 5, 1871.—Prof. Morris, Vice-President, in the Chair.

THE following communications were read :—

1. " On a new Chimæroid Fish from the Lias of Lyme Regis." By Sir Philip Grey Egerton, Bart., M.P., F.R.S., V.P.G.S.

This fish, for which the author proposed the name of *Ischyodus orthorhinus*, was represented by a specimen showing the anterior structures imbedded in a slab of Lias. It exhibited the charac- teristic dental apparatus of the Chimæroids, surrounded with sha- green, a very large prelabial appendage (six inches long, and termi- nating in a hook abruptly turned downwards), and a process which the author regarded as representing the well-known rostral appen- dage of the male Chimæroid, but in this case attaining a length of $5\frac{1}{2}$ inches, and covered more or less thickly with tubercles, bearing recurved central spines somewhat tooth-like in their aspect. This appendage is attached to the head by a rounded condyle, received into a hollow in the frontal cartilage. The dorsal spine, which measured 6 inches in length, was articulated by a rounded surface to a strong cartilaginous plate projecting upwards from the noto- chordal axis, and was thus rendered capable of a considerable amount of motion in a vertical plane. This structure also occurred in *Callorhynchus* and *Chimæra*.

2. " On the Tertiary Volcanic Rocks of the British Islands." By Archibald Geikie, Esq., F.R.S., F.G.S., Director of the Geological Survey of Scotland, and Professor of Geology and Mineralogy in the University of Edinburgh.—First paper.

In this communication the author gave the first of a series of

papers which he proposes to lay before the Society upon the volcanic rocks of Britain of later date than the Chalk. In a general introduction to the whole subject, he pointed out the area occupied by the rocks, showing that they are chiefly developed along the broad tract which extends from the south of Antrim, between the chain of the Outer Hebrides and the mainland of Scotland, up into the Faroe Islands, and even to Iceland. The nomenclature of the rocks was discussed, and the following arrangement was proposed :—

	Felspathic series.					Pyroxenic, or Augitic.					
	Syenite.	Felstone and Quartz-porphyry.	Trachyte and Trachyte-porphyry.	Pitchstone.	Porphyrite.	Dolerite.	Basalt.	Trachylite.	Diallage-rock (? altered dolerite.)	Felspathic tuffs.	Pyroxenic tuffs and agglomerate.
I. INTERBEDDED OR CONTEMPORANEOUS.											
A. *Crystalline.*											
Sheets or beds	...	...	?	*	*	*	*	...	*		
B. *Fragmental.*											
Beds or layers	...	...	...	...	...	...	...	...	...	?	*
II. INTRUSIVE OR SUBSEQUENT.											
A. *Crystalline.*											
α. Amorphous masses	*	*	?	...	...	?					
β. Sheets	...	...	...	...	...	*	*				
γ. Dykes and veins	*	*	?	*	...	*	*	*			
δ. Necks..........................	...	?	...	...	...	...	?				
B. *Fragmental.*											
Necks..............................	...	...	...	...	...	...	...	...	...	...	*

The age of the rocks was shown to be included in the Tertiary period by the position of the volcanic masses above the Chalk, and by their including beds containing Miocene plants.

As an illustrative district, the author described the volcanic geology of the Island of Eigg, one of the Inner Hebrides, and brought out the following points :—

1. The volcanic rocks of this island rest unconformably upon strata of Oolitic age.

2. They consist almost wholly of a succession of nearly horizontal interbedded sheets of dolerite and basalt, forming an isolated fragment of the great volcanic plateau which stretches in broken masses from Antrim through the Inner Hebrides.

3. These interbedded sheets are traversed by veins and dykes of similar materials, the dykes having the characteristic north-westerly trend with which they pass across the southern half of Scotland and the north of England. Veins of pitchstone and felstone, and intru-

sive masses of quartziferous porphyry, like some of those which in Skye traverse or overlie the lias, likewise intersect the bedded dolerites and basalts of Eigg.

4. At least two widely separated epochs of volcanic activity are represented by the volcanic rocks of Eigg. The older is marked by the bedded dolerites and by the basalt veins and dykes which, though strictly speaking younger than the bedded sheets which they intersect, yet probably belong to the same continuous period of volcanic action. The later manifestations of this action are shown by the pitchstone of the Scùr. Before that rock was erupted, the older doleritic lavas had long ceased to flow in this district. Their successive beds, widely and deeply eroded by atmospheric waste, were here hollowed into a valley traversed by a river, which carried southward the drainage of the wooded northern hills. Into this valley, slowly scooped out of the older volcanic series, the pitchstone and porphyry *coulées* of the Scùr flowed. Vast, therefore, as the period must be which is chronicled in the huge piles of volcanic beds forming our dolerite plateaux, we must add to it the time needed for the excavation of parts of those plateaux into river-valleys, and the concluding period of volcanic activity during which the rocks of the Scùr of Eigg were poured out.

5. Lastly, from the geology of this interesting island we learn, what can be nowhere in Britain more eloquently impressed upon us, that, geologically recent as that portion of the Tertiary period may be during which the volcanic rocks of Eigg were produced, it is yet separated from our own day by an interval sufficient for the removal of mountains, the obliteration of valleys, and the excavation of new valleys and glens where the hills then stood. The amount of denudation which has taken place in the Western Islands since Miocene times will be hardly credible to those who have not adequately realized the potency and activity of the powers of geological waste. Subterranean movements may be called in to account for narrow gorges, or deep glens, or profound sea-lochs; but no subterranean movement will ever explain the history of the Scùr of Eigg, which will remain as striking a memorial of denudation as it is a landmark amid the scenery of our wild western shores.

3. " On the formation of 'Cirques,' and their bearing upon theories attributing the excavation of Alpine Valleys mainly to the action of Glaciers." By the Rev. T. G. Bonney, M.A., F.G.S.

The paper described a number of these remarkable recesses, which, though not restricted to the limestone districts of the Alps, are best exhibited in them. The author gave reasons why he could not suppose them to have been formed either as craters of upheaval, or by the action of the sea, or by glacial erosion. With regard to the last he showed that, even if glaciers had been the principal agents in excavating valleys, there were some cirques which could not have been excavated by them; and then went on to argue, from the fact that glaciers had occupied cirques, and from the relation between them and the valleys, that they could not be attributed to different

agents. He also showed that commonly the upper part of a valley, where the erosive action is perhaps least, is very much the steepest, and urged other objections to the great excavatory powers often attributed to glaciers. He then described one or two cirques in detail, and showed that they were worked out by the joint action of many small streams, and of the usual meteoric agents working upon strata whose configuration was favourable to the formation of cliffs.

XXXIX. *Intelligence and Miscellaneous Articles.*

ON THE SPECTRA OF SULPHUR. BY M. G. SALET.

THE employment of instruments and methods the delicacy and per-
fection of which greatly surpass those of our organs has often entailed difficulties and errors. When spectral analysis is applied to the minute quantities of matter which fill the Geissler tubes, we often find ourselves in the presence of impurities which can only be revealed by the spectral method itself; hence uncertainties. These impurities may proceed not merely from the primitive gas, but also from that operated on previously with the mercurial air-pump, from the mercury of the pump, from the grease of the stopcocks, from the sulphuric acid used as desiccating agent, from matters deposited on the surface of the glass, and from the metallic electrodes, which possess the property of absorbing and afterwards diffusing a certain number of gases. One can easily conceive how M. Ångström, by indicating these impurities, recently arrived at the suppression, as not belonging to the pure gas, of all the supplementary spectra of hydrogen described by M. Wüllner. But it seems to me that the discovery of M. Plücker is not shaken by these facts, or that, at least as regards sulphur, which I have studied, there really exist two perfectly distinct spectra—one composed of lines, the other of bands, and both characteristic of the same degree. The first is obtained by the disruptive discharge; the second can be produced by discharges of less tension, by the incandescence of sulphur in the hydrogen-flame, and, finally, though with less distinctness, by the absorption of sulphur vapour alone.

1. *The Electric Spectrum.*—I enclose the sulphur in a glass tube similar to M. Plücker's, but not presenting metallic electrodes. Each extremity of the tube is surrounded by a brass sheath, which is heated by means of a lamp in order to vaporize the sulphur. When we wish the electricity to pass, we connect the sheaths with the poles of a coil or a Holtz machine; and by influence the tube becomes as intensely luminous as if the electrodes penetrated the interior. When an excellent vacuum has been made in the apparatus during the vaporization of a large portion of the sulphur, which distilled outside, there need be no fear of the presence of a foreign gas; besides, when such a tube is cold, the electricity no longer circulates; and, on employing metallic electrodes, even a Geissler's tube in which a vacuum has been made over boiling sulphur arrests the spark perfectly. The following are the wave-lengths of the middle of each band observed in the spectrum obtained in this manner by heating moderately, and employing electricity of feeble tension :—

406 very broad and vague.	467 strong.	504·5 strong.	548 strong.
418 the same.	470	508·5	554
431·5 strong.	475	515	560
434·5 strong.	479	522 strong.	564
445	483	526 strong.	570
448 strong.	487·5	532	577
453·5	492	538	581
462	498	544	590

No hydrogen or nitrogen-band is visible.

2. *Spectrum in the flame of Hydrogen.*—This I produced by pressing the hydrogen-flame, charged with traces of sulphurous acid, against a layer of cold water falling vertically. The beautiful blue light then produced is easily resolved by the prism into bands quite similar to the preceding, but some of which are brighter than the corresponding bands of the electric spectrum, so as at first sight to present a somewhat different appearance. Their wave-lengths were:—

396 very broad and vague.	438·5	471 strong.	504
404 the same.	444·5 strong.	476	509
408·5 vague.	448	479	515
416	453·5	483	faint lines
419	457·5	487·5	as far as to
427 strong.	462	492	550
431·5 strong.	467	498 strong.	

3. I examined by transparency a layer of sulphur vapour heated to dull redness. When a very powerful light is employed, such as that of magnesium, in the blue some black bands are perceived which nearly correspond to the following wave-lengths:—

471   465   462 very faint.   456   445   437

Here there may be some uncertainty, because magnesium furnishes some lines in this portion of the spectrum; nevertheless, as these bands are only observed with sulphur, I think they are due to the reversal of the preceding spectrum.

All these results were obtained with a spectroscope with only one prism, as the band spectra, unlike the line spectra, will not bear much dilatation; therefore one cannot absolutely depend on the number which expresses the millionths of a millimetre.

I am pursuing the study of the band spectra of the metalloids, in M. Wurtz's laboratory.— *Comptes Rendus*, 1871, No. 9.

ON SOME LUMINOUS TUBES WITH EXTERIOR ELECTRODES.
NOTE BY M. ALVERGNIAT.

The presence of metallic electrodes (which sometimes become strongly heated) in Geissler tubes may occasion numerous misconceptions when the light is analyzed in the spectroscope. Those electrodes absorb and emit gases, may cause cracks in the glass; and they partly volatilize, so as to tarnish the interior surface of the latter. Now it is not necessary to place the electrodes inside the tubes, since these can be charged by influence (as we have seen in the preceding note) without their brightness being much diminished.

Besides, the exterior electrode need not be a collar; it may be

formed by a glass tube open outside and entering the interior of the apparatus ; it is into this tube that the metallic electrode is intro- duced which serves for the passage of the discharge.

The phenomenon of stratification can thus be observed very distinctly. When the coil is in operation, the production of a large quantity of ozone around the apparatus is observed ; I intend to take advantage of this peculiarity in order to construct an apparatus suitable for the production of that gas.—*Comptes Rendus,* 1871, No. 9.

ON THE AMOUNT OF TIME NECESSARY FOR VISION.   BY OGDEN N. ROOD, PROFESSOR OF PHYSICS IN COLUMBIA COLLEGE.

In the celebrated experiment of Wheatstone on the duration of the discharge of a Leyden jar, the conclusion was drawn that distinct vision is possible in less than one millionth of a second.   The in- correctness of the data on which this conclusion rested was after- ward pointed out in an admirable investigation by Feddersen, who remarks on this point :—" One cannot hereafter assume in optical and physiological experiments that the discharge of a Leyden jar is an instantaneous act ; but at the same time, by the determination of the greatest suitable resistance, it will be possible to limit the discharge to its least possible duration " *.   The smallest measured duration obtained by Feddersen was one millionth of a second.

In an article in Silliman's Journal for September 1871 I show how, by the use of a much smaller electrical surface, I obtained and mea- sured sparks the duration of whose main constituent was only forty billionths of a second.   With their light distinct vision is possible ; thus, for example, the letters on a printed page are plainly to be seen ; also, if a polariscope be used, the cross and rings around the axes of crystals can be observed with all their peculiarities, and errors in the azimuth of the analyzing prism noticed.   There seems also to be evidence that this minute interval of time is sufficient for the production of various subjective optical phenomena—for ex- ample, for the recognition of Loewe's rings (using cobalt glass); also the radiating structure of the crystalline lens can be detected when the light is suitably presented to the eye.

Hence it is plain that forty billionths of a second is quite sufficient for the production on the retina of a strong and distinct impression ; and as the obliteration of the micrometric lines in the experiment referred to could only take place from the circumstance that the retina retains and combines a whole series of impressions whose *joint duration* is forty billionths of a second, it follows that a much smaller interval of time will suffice for vision.   If we limit the number of views of the lines presented to the eye in a single case to ten, it would result that four billionths of a second is sufficient for human vision, though the probability is that a far shorter time would answer as well, or nearly as well.   All of which is not so wonderful if we accept the doctrines of the undulatory theory of light ; for ac- cording to it, in four billionths of a second nearly two and a half millions of the mean undulations of light reach and act on the eye. —Silliman's *American Journal,* September 1871.

* Pogg. *Ann.* vol. cxiii. p. 453.

XL. *On the Application of a Mechanical Equation advanced by me to the Motion of a Material Point round a fixed Centre of Attraction, and of two Material Points about each other.* By R. CLAUSIUS[*].

I HAVE recently, in investigations on the mechanical theory of heat, advanced a new equation relative to stationary motion[†], which is connected with the theorem of the minimum effect, but extends to cases to which that theorem does not apply. I will here introduce the equation only in the forms which are suitable for the considerations here intended, referring the reader to my earlier memoir for further transformations.

Given a material point moving, under the influence of a given force, stationarily in a closed path. Let m be the mass of this point, x, y, z its coordinates referred to a rectangular system, X, Y, Z the components of the force operating on it, v its velocity, and i the time of a revolution. All these quantities, except the first and the last, are variable in the course of the motion; but each has a certain mean value for the entire revolution. We will distinguish such a value from the variable quantity by putting a horizontal stroke over the sign which represents the latter; so that, for example, $\bar{x}$ will signify the mean value of x.

Now let us imagine the original motion replaced by another,

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[*] Translated from a separate impression, communicated by the Author, from the *Nachrichten von der Königlichen Gesellschaft der Wissenschaften*, 1871, No. 8.

[†] " Ueber die Zurückführung des zweiten Hauptsatzes der mechanischen Wärmetheorie auf allgemeine mechanische Principien," *Sitzungsberichte der Niederrheinischen Gesellschaft für Natur- und Heilkunde*, 1870, p. 167 ; Pogg. *Ann.* vol. cxlii. p. 433 ; Phil. Mag. S. 4. vol. xlii. p. 161.

infinitesimally different, stationary motion, which may take place in a different but likewise closed path, with another velocity, and under the influence of another force. In comparing these two motions, we will name the difference between a quantity relative to the original motion and the corresponding quantity relative to the altered motion the *variation* of the quantity, and denote it by a prefixed δ; so that, for instance, δ*i* will be the variation of the time of revolution *i*. But with respect to the quantities which are variable in the course of every motion, it is necessary to settle which values are to be regarded as corresponding values of the quantity. This may be done in the following manner. We first take, in the two paths, two places infinitely near each other as corresponding places, and reckon the periods of motion from the moments when the point passes through these places. Then, denoting by *t* the time of motion till any other place in the path is reached, with the original motion we put

$$t = i\phi,$$

and with the altered motion we put

$$t + \delta t = (i + \delta i)\phi,$$

in which ϕ is a variable which I have named the *phase* of the motion, and, in both motions, increases from 0 to 1 during a revolution. If, now, ϕ has the same value in both these equations, *t* and $t + \delta t$ are corresponding values of the times of motion. Thence follow further the corresponding places in the two paths, and the corresponding values of all the other quantities which refer to the two motions.

From these explanations the following equation (which is the simplest form of the one above mentioned) will be intelligible:—

$$- \overline{(X\delta x + Y\delta y + Z\delta z)} = \frac{m}{2} \overline{\delta v^2} + m\overline{v^2} \delta \log i. \quad . \quad . \quad (1)$$

When the force which operates on the moving point has an *ergal* —in other words, when the components of the force can be represented by the partial differential coefficients of a function of the coordinates of the point taken negatively, which may be denoted by U, the equation is transformed into

$$\overline{\frac{dU}{dx} \delta x + \frac{dU}{dy} \delta y + \frac{dU}{dz} \delta z} = \frac{m}{2} \overline{\delta v^2} + m\overline{v^2} \delta \log i*. \quad . \quad (2)$$

* In my above-cited earlier memoir I have investigated under what circumstances the left-hand side of this equation may be valid as the expression of the mechanical work done in the transition from the one stationary motion to the other. Into that question we need not enter here, since, for our present investigations, we have not to trace the passage from one stationary motion to another, but only to compare two given stationary motions differing infinitely little the one from the other.

The sum

$$\frac{d\mathrm{U}}{dx}\,\delta x + \frac{d\mathrm{U}}{dy}\,\delta y + \frac{d\mathrm{U}}{dz}\,\delta z$$

must not at once be regarded as the variation of the ergal, and hence, if the signification of U be extended so that it shall represent the ergal not only for the original, but also for the altered motion, cannot at once be denoted by δU. The preceding equation holds good, namely, as already intimated, also for the cases in which the force operating on the point has undergone a variation which can be supposed to be mathematically expressed thus:—that one or more quantities contained in the ergal, constant during one stationary motion, have different values in the two motions. In such a case, of course, in the determination of the variation δU the difference of the constants must be taken into account in addition to the difference of the coordinates.

But we will now assume that, in the two motions which we have now to compare, such a difference does not occur—the ergal in both being represented by one and the same function of the coordinates, with unaltered constants. In this case the above sum is the complete variation of the ergal and can be denoted by δU; and accordingly the left-hand side of equation (2) is the mean value of the variation of the ergal, or (which is the same thing) the variation of its mean value, which is represented by δŪ. For this case, therefore, equation (2) passes into

$$\delta\bar{\mathrm{U}} = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\,\delta\log i. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

To this equation we will now give a somewhat simpler form. We will first transform it in the following manner:—

$$\delta\bar{\mathrm{U}} = m\overline{v^2}\left(\tfrac{1}{2}\frac{\delta\overline{v^2}}{\overline{v^2}} + \delta\log i\right)$$

$$= m\overline{v^2}(\tfrac{1}{2}\delta\log\overline{v^2} + \delta\log i)$$

$$= m\overline{v^2}\,\delta\log(i\sqrt{\overline{v^2}}).$$

For the product here standing under the logarithm, we will introduce a single sign, putting

$$\lambda = i\sqrt{\overline{v^2}}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

Then our equation is transformed into

$$\delta\bar{\mathrm{U}} = m\overline{v^2}\,\delta\log\lambda. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

As the left-hand side of this equation is a variation, the right-hand side must be so too. Hence it follows that $m\overline{v^2}$ is a function of λ; and accordingly Ū must then be also a function of λ.

Y 2

For the latter we will first introduce the sign of any function, putting

$$\overline{U} = f(\lambda). \quad \dots \quad \dots \quad (6)$$

The other function also, represented by $m\overline{v^2}$, can then be immediately specified; namely, if $f'(\lambda)$ signifies the first derivative from $f(\lambda)$, it is

$$\delta\overline{U} = f'(\lambda)\delta\lambda.$$

If we introduce this product into equation (5), and at the same time replace the variation of the logarithm by its value, we obtain

$$f'(\lambda)\delta\lambda = m\overline{v^2}\frac{\delta\lambda}{\lambda},$$

whence follows

$$m\overline{v^2} = \lambda f'(\lambda);$$

or, dividing by 2,

$$\frac{m}{2}\overline{v^2} = \tfrac{1}{2}\lambda f'(\lambda). \quad \dots \quad \dots \quad (7)$$

Further, from (4) we will form the following equation:—

$$i = \frac{\lambda}{\sqrt{\overline{v^2}}}.$$

Putting here for $\overline{v^2}$ the value which results from the preceding equation, we have

$$i = \sqrt{\frac{m\lambda}{f'(\lambda)}}. \quad \dots \quad \dots \quad (8)$$

Finally, we will represent yet a fourth quantity by λ. According to the theorem of the equivalence of *vis viva* and mechanical work, we have the equation

$$U + \frac{m}{2}v^2 = E,$$

in which E is a quantity which is constant during the motion, and which we will call the *energy*. If the sum of the two variables on the left-hand side of this equation has a constant value during the entire motion, the sum of their mean values has the same value, and hence we can write

$$E = \overline{U} + \frac{m}{2}\overline{v^2}.$$

By inserting here the expressions from (6) and (7), we obtain

$$E = f(\lambda) + \tfrac{1}{2}\lambda f'(\lambda). \quad \dots \quad \dots \quad (9)$$

Consequently, as soon as the form of the function $f(\lambda)$ is known, we can, by virtue of equations (6), (7), (8), and (9), express by

one and the same quantity λ the mean ergal, the mean *vis viva*, the time of a revolution, and the energy. It is of course under-stood that we eliminate λ from each two of these equations, and can thereby obtain relations between each two of the four quan-tities named. Let us suppose this carried out by combining each of the first three equations with the last; we thus obtain three equations, which determine the mean ergal, the mean *vis viva*, and the time of a revolution as functions of the energy. This method of determination is peculiarly convenient for appli-cation, inasmuch as in every motion the energy has a constant value, which can be at once given, if only the velocity of the moving point in any position is known.

We now require only to find the form of the function $f(\lambda)$. This, of course, depends on the law under which the force ope-rates on the point. The determination of this function is espe-cially easy when the force is *an attractive force proceeding from a fixed centre, and is represented by any function of the distance*; and this case we will now consider*.

The distance of the moving point from the centre of attraction may be denoted by r, and the function which represents the quantity of the force by $F'(r)$. If we then put

$$f\,F'(r)dr = F(r), \quad \ldots \quad \ldots \quad (10)$$

$F(r)$ is the ergal; and by introducing this function in the place of U, equation (6) is transformed into

$$\overline{F(r)} = f(\lambda). \quad \ldots \quad \ldots \quad \ldots \quad (11)$$

If now for any special case of the motion the form of the func-tion which satisfies this equation can be found, the same form will also hold good universally. Such a case is when the point moves round the centre of attraction in a *circular* path. r is then constant, and we need not take the mean value of $F(r)$, but can write simply

$$F(r) = f(\lambda). \quad \ldots \quad \ldots \quad \ldots \quad (12)$$

Further, in this case the velocity also is constant; and hence in equation (4) we can, in the place of the mean value $\overline{v^2}$, put simply v^2, by which it is transformed into

$$\lambda = i\sqrt{v^2} = iv.$$

When, however, the velocity is constant, the product iv is equal

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* As the motion of a material point under the influence of a central force need not take place in a closed path, I will once more give prominence to the fact that the following formulæ are to be referred only to motions which take place in closed paths. For the application of my equation to other motions, further special analyses would be necessary, which would carry us beyond our present limits.

to the length of the path; and since in our case the path is a circle with the radius r, we obtain

$$\lambda = 2\pi r,$$

or

$$r = \frac{\lambda}{2\pi}.$$

This, inserted in equation (12) for r, gives

$$F\left(\frac{\lambda}{2\pi}\right) = f(\lambda). \quad \ldots \quad \ldots \quad (13)$$

Hereby the function $f(\lambda)$ is determined. By differentiation according to λ we obtain further:—

$$\frac{1}{2\pi} F'\left(\frac{\lambda}{2\pi}\right) = f'(\lambda). \quad \ldots \quad \ldots \quad (14)$$

For simplicity, we will now introduce the new sign ρ with the signification

$$\rho = \frac{\lambda}{2\pi}; \quad \ldots \quad \ldots \quad \ldots \quad (15)$$

we then obtain

$$f(\lambda) = F(\rho), \quad \ldots \quad \ldots \quad \ldots \quad (16)$$

$$f'(\lambda) = \frac{1}{2\pi} F'(\rho), \quad \ldots \quad \ldots \quad (17)$$

$$\lambda f'(\lambda) = \rho F'(\rho). \quad \ldots \quad \ldots \quad \ldots \quad (18)$$

Applying this to equation (11), which has taken the place of (6), and to equations (7), (8), and (9), we arrive, for the case in which the effective force is an attraction proceeding from a fixed centre and represented by a function of the distance, at the following equations:—

$$\overline{F(r)} = F(\rho), \quad \ldots \quad \ldots \quad \ldots \quad (19)$$

$$\frac{m}{2} \overline{v^2} = \tfrac{1}{2} \rho F'(\rho), \quad \ldots \quad \ldots \quad (20)$$

$$i = 2\pi \sqrt{\frac{m\rho}{F'(\rho)}}, \quad \ldots \quad \ldots \quad (21)$$

$$E = F(\rho) + \tfrac{1}{2} \rho F'(\rho), \quad \ldots \quad \ldots \quad (22)$$

wherein all the functions are known.

As a still more special case, we will assume that the attractive force is proportional to any positive or negative power of the distance; but we will except the minus first power, which, in the integration, leads to the logarithm, and hence it will be better to treat it specially. We therefore put

$$F'(r) = kr^n, \quad \ldots \quad \ldots \quad \ldots \quad (23)$$

in which k and n are constants, and the latter is different from -1. From this, through integration, results

$$F(r) = \frac{k}{n+1} r^{n+1}; \quad \ldots \quad \ldots \quad (24)$$

and by employing these forms of the functions the above four equations are transformed into :—

$$\frac{k}{n+1} \overline{r^{n+1}} = \frac{k}{n+1} \rho^{n+1}, \quad \ldots \quad \ldots \quad (25)$$

$$\frac{m}{2} \overline{v^2} = \frac{k}{2} \rho^{n+1}, \quad \ldots \quad \ldots \quad \ldots \quad (26)$$

$$i = 2\pi \sqrt{\frac{m}{k}} \cdot \overline{\rho^{\frac{1-n}{2}}}, \quad \ldots \quad \ldots \quad (27)$$

$$E = k \frac{n+3}{2(n+1)} \rho^{n+1}. \quad \ldots \quad \ldots \quad (28)$$

Eliminating ρ from the first three equations by means of the last, we obtain :—

$$\frac{k}{n+1} \overline{r^{n+1}} = \frac{2}{n+3} E, \quad \ldots \quad \ldots \quad \ldots \quad (29)$$

$$\frac{m}{2} \overline{v^2} = \frac{n+1}{n+3} E, \quad \ldots \quad \ldots \quad \ldots \quad (30)$$

$$i = 2\pi m^{\frac{1}{2}} k^{-\frac{1}{n+1}} \left[\frac{2(n+1)}{n+3} E \right]^{\frac{1-n}{2(n+1)}}. \quad . \quad (31)$$

In order, finally, still further to specialize, we will put for n two definite values which most frequently occur.

First, we will assume n to be equal to 1. This case corresponds to the most simple elastic vibratory motions, in which the force with which a point that has left its position of equilibrium is drawn back toward it is proportional to the distance. For this case the preceding equations are transformed into :—

$$\frac{k}{2} \overline{r^2} = \frac{k}{2} \rho^2 = \tfrac{1}{2} E, \quad \ldots \quad \ldots \quad (32)$$

$$\frac{m}{2} \overline{v^2} = \frac{k}{2} \rho^2 = \tfrac{1}{2} E, \quad \ldots \quad \ldots \quad (33)$$

$$i = 2\pi \sqrt{\frac{m}{k}}. \quad \ldots \quad \ldots \quad (34)$$

The last equation expresses that the time of revolution is independent of the elongation of the vibrations, and consequently that these are isochronous.

Secondly, it shall be assumed that $n = -2$, which corresponds to Newton's law of attraction, that governs the motion of the heavenly bodies. For this case the preceding equations become:—

$$-k\frac{\overline{1}}{r} = -k\frac{1}{\rho} = 2\mathrm{E}, \quad \ldots \ldots \ldots \quad (35)$$

$$\frac{m}{2}\overline{v^2} = \frac{k}{2}\cdot\frac{1}{\rho} = -\mathrm{E}, \quad \ldots \ldots \quad (36)$$

$$i = 2\pi\sqrt{\frac{m}{k}}\cdot\rho^{\frac{3}{2}} = 2\pi k\sqrt{m}(-2\mathrm{E})^{-\frac{3}{2}}. \quad (37)$$

The last equation, which may also be written

$$i^2 = (2\pi)^2\frac{m}{k}\rho^3,$$

corresponds to Kepler's third law, which is contained in our equations as a special case. It must, however, be somewhat otherwise expressed than it was by Kepler and frequently still is, namely that the *squares of the periods are as the cubes of the mean distances*. This is not strictly correct; for ρ is not the mean value of r, but $\dfrac{1}{\rho}$ is the mean value of $\dfrac{1}{r}$. The other, more accurate form of the theorem, that the *squares of the periods are as the cubes of the major axes of the ellipses,* agrees perfectly with our equation; for it can easily be proved that ρ is equal to half the major axis of the ellipse which forms the path when the central force is of this kind.

We now turn to the motion of two material points about each other.

Let us first assume that we have any number of material points which move in a stationary manner in closed paths, and that these motions undergo an infinitely small alteration, so that stationary motions in closed paths again arise; my equation reads for this case:—

$$-\overline{\Sigma(\mathrm{X}\delta x + \mathrm{Y}\delta y + \mathrm{Z}\delta z)} = \Sigma\frac{m}{2}\,\overline{\delta v^2} + \Sigma m\overline{v^2}\,\delta\log i. \quad (38)$$

If the forces operating on the points are such that they have an ergal, which we will denote by U, and if we again suppose that with the alteration of the motion the ergal remains an invariable function of the coordinates of all the points, then we can put, corresponding to equation (3):—

$$\delta\mathrm{U} = \Sigma\frac{m}{2}\,\overline{\delta v^2} + \Sigma m v^2\,\delta\log i. \quad \ldots \quad (39)$$

If the forces operating in our system consist only of the attrac-

tions and repulsions which the moving points exert on one another, and which, according to any law, depend on the distance, it is known that the ergal can be expressed very simply. If the force which two points whose masses are m and m_1 exert on one another at the distance r is represented by $mm_1\phi'(r)$, in which a positive value of the function corresponds to an attraction—and if, further,

$$\phi(r) = f\phi'(r)dr,$$

then the ergal is determined by the equation

$$U = \Sigma mm_1\phi(r),$$

in which the summation embraces all the combinations of the given points in pairs. Accordingly, for this case the preceding equation is transformed into

$$\delta\Sigma mm_1\overline{\phi(r)} = \Sigma\frac{m}{2}\,\delta\overline{v^2} + \Sigma m\overline{v^2}\,\delta\log i. \quad . \quad . \quad (40)$$

We will now assume, specially, that only two material points are given, the masses of which are m and m_1, and which move about each other under the influence of their mutual attraction. In this case, if we denote by letters to which an index is attached all the quantities which relate to the second point, we can write the preceding equation without signs of summation, thus:—

$$mm_1\delta\overline{\phi(r)} = \frac{m}{2}\,\delta\overline{v^2} + \frac{m_1}{2}\,\delta\overline{v_1^2} + m\overline{v^2}\delta\log i + m_1\overline{v_1^2}\delta\log i_1.$$

Since, however, in such a motion of two points about each other, both points have the same period, $i_1 = i$, and hence the last two terms can be combined. By bringing at the same time the first two terms on the right-hand side under a common sign of variation, we can write:—

$$mm_1\delta\overline{\phi(r)} = \tfrac{1}{2}\delta(\overline{mv^2} + m_1\overline{v_1^2}) + (m\overline{v^2} + m_1\overline{v_1^2})\delta\log i. \quad . \quad (41)$$

To this equation we can give a still more simplified form. For this purpose the relative velocity u of the two points may be introduced; it is determined by the following equation:—

$$u^2 = \left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 + \left(\frac{dy}{dt} - \frac{dy_1}{dt}\right)^2 + \left(\frac{dz}{dt} - \frac{dz_1}{dt}\right)^2. \quad . \quad (42)$$

But now it may be readily seen by resolution of the bracketed terms that the following equation holds good:—

$$mm_1\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 = (m + m_1)\left[m\left(\frac{dx}{dt}\right)^2 + m_1\left(\frac{dx_1}{dt}\right)^2\right] - \left(m\frac{dx}{dt} + m_1\frac{dx_1}{dt}\right)^2.$$

On the condition we have supposed, that the two points move

only under their reciprocal action, in closed paths, about each other, their common centre of gravity must remain fixed, and hence

$$m\frac{dx}{dt} + m_1\frac{dx_1}{dt} = 0,$$

by which the preceding equation is transformed into

$$mm_1\left(\frac{dx}{dt} - \frac{dx_1}{dt}\right)^2 = (m + m_1)\left[m\left(\frac{dx}{dt}\right)^2 + m_1\left(\frac{dx_1}{dt}\right)^2\right].$$

Exactly similar equations are valid for the y and z directions; and if we suppose these three equations added, we obtain

$$mm_1u^2 = (m + m_1)(mv^2 + m_1v_1^2),$$

or

$$mv^2 + m_1v_1^2 = \frac{mm_1}{m + m_1}u^2. \quad \ldots \quad (43)$$

If this value of $mv^2 + m_1v_1^2$ be introduced into equation (41), and then the product mm_1 be taken away, the result will be:—

$$\delta\overline{\phi(r)} = \frac{1}{m + m_1}\left(\tfrac{1}{2}\delta\overline{u^2} + \overline{u^2}\,\delta\log i\right). \quad \ldots \quad (44)$$

For still further abbreviation we will write this equation in the following form:—

$$\delta\overline{\phi(r)} = \frac{\overline{u^2}}{m + m_1}\,\delta\log\left(i\sqrt{\overline{u^2}}\right). \quad \ldots \quad (45)$$

And here we will again introduce a single symbol for the product which stands under that of the logarithm, putting

$$\lambda = i\sqrt{\overline{u^2}}, \quad \ldots \quad \ldots \quad \ldots \quad (46)$$

by which we obtain

$$\delta\overline{\phi(r)} = \frac{\overline{u^2}}{m + m_1}\,\delta\log\lambda. \quad \ldots \quad \ldots \quad (47)$$

This equation can now be treated just as equation (5) was. The left-hand side being a variation, the right-hand side must be so too; and hence $\overline{u^2}$ must be a function of λ; and thence it follows further that $\overline{\phi(r)}$ must also be a function of λ. Hence we put preliminarily

$$\overline{\phi(r)} = f(\lambda), \quad \ldots \quad \ldots \quad (48)$$

and put to ourselves the question whether, for any special kind of motion, the form of the function $f(\lambda)$ can be found. This can be done when the two points so move about each other that

their mutual distance remains constant. Then we need not take the mean value of $\phi(r)$, but can write

$$\phi(r) = f(\lambda). \quad . \quad . \quad . \quad . \quad . \quad . \quad (49)$$

Further, for this case u also is constant, and equation (46) changes into

$$\lambda = i\sqrt{\overline{u^2}} = iu.$$

The product iu here occurring has a simple signification; for it is the relative length of the path—that is, the length we obtain when we conceive one of the points at rest and ascribe the velocity u to the other. This path is a circle with the radius r; and hence we obtain

$$\lambda = iu = 2\pi r,$$

and consequently

$$r = \frac{\lambda}{2\pi}.$$

This value inserted in equation (49) gives

$$\phi\left(\frac{\lambda}{2\pi}\right) = f(\lambda); \quad . \quad . \quad . \quad . \quad . \quad (50)$$

and by this the form of the function $f(\lambda)$ is determined. If, as before, we introduce ρ with the signification

$$\rho = \frac{\lambda}{2\pi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (51)$$

there results

$$\phi(\rho) = f(\lambda);$$

and by employing this equation, (48) is transformed into

$$\overline{\phi(r)} = \phi(\rho). \quad . \quad . \quad . \quad . \quad . \quad . \quad (52)$$

Returning now to equation (47), according to what precedes we can write it in the following form :—

$$\delta\phi(\rho) = \frac{\overline{u^2}}{m + m_1} \delta \log(2\pi\rho),$$

or

$$\phi'(\rho)\delta\rho = \frac{\overline{u^2}}{m + m_1} \cdot \frac{\delta\rho}{\rho},$$

from which follows :—

$$\overline{u^2} = (m + m_1)\rho\phi'(\rho). \quad . \quad . \quad . \quad . \quad (53)$$

When, further, in equation (46), in the place of λ we put the product $2\pi\rho$, there comes

$$2\pi\rho = i\sqrt{\overline{u^2}},$$

or

$$i = 2\pi \frac{\rho}{\sqrt{\overline{u^2}}}.$$

Inserting here for $\overline{u^2}$ its value from (53) gives

$$i = 2\pi \sqrt{\frac{\rho}{(m + m_1)\phi'(\rho)}}. \quad \cdot \quad \cdot \quad \cdot \quad (54)$$

Lastly, we will express the energy of our system.   It is

$$E = m m_1 \phi(r) + \frac{m}{2} v^2 + \frac{m_1}{2} v_1^2$$

$$E = m m_1 \phi(r) + \tfrac{1}{2} \frac{m m_1}{m + m_1} u^2,$$

and consequently also

$$E = m m_1 \overline{\phi(r)} + \tfrac{1}{2} \frac{m m_1}{m + m_1} \overline{u^2}.$$

The values from (52) and (53) inserted here give

$$E = m m_1 [\phi(\rho) + \tfrac{1}{2}\rho\phi'(\rho)]. \quad \cdot \quad \cdot \quad \cdot \quad (55)$$

We have thus again arrived at a system of four equations—
(52), (53), (54), and (55)—by means of which we represent by
ρ the mean ergal, the mean *vis viva*, the period, and the energy;
or else, after eliminating ρ, we can express the first three of these
quantities as functions of the energy, therefore as functions of a
quantity whose value can be given as soon as, for any distance of
the two points, their relative velocity is known.

The four equations here found are of the same form as the
equations (19) to (22)—which might have been expected, since the
motion first considered is only a special case of the one last dis-
cussed—namely, the limiting case, which is arrived at by assu-
ming that one of the masses is so large in proportion to the other,
that during the motion round their common centre of gravity it
can be regarded as at rest.   Hence it will not be necessary to
develope the special forms which the equations here found
assume when the force is proportional to a power of the distance,
because they perfectly correspond to the forms previously de-
veloped.

XLI. *On* Canon Moseley's *views upon Glacier-motion.*
By WILLIAM MATHEWS, *President of the Alpine Club*[*].

IN the Philosophical Magazine for August 1871 Canon
Moseley has criticised some remarks of mine upon the be-
haviour of ice-planks under strains produced by their own gravi-
tation.   The remarks in question were contributed to the Alpine

Journal for February, and to 'Nature' for March 24, 1870, where it was pointed out that an ice-plank 6 inches wide and 2 inches thick, or thereabout, when supported upon horizontal bearers 6 feet apart, became gradually deflected, even under a temperature constantly below freezing, and was just as rigid in its altered as in its original form. I regarded this experiment as absolutely subversive of the Canon's theory, that the descent of glaciers by their weight alone is a mechanical impossibility, and in the article in 'Nature' expressed myself as follows :—" I shall not now attempt to discuss the nature of the molecular displacements to which the change of form is due. *Their occurrence is indisputable,* and whether or not they are dignified by the name of *shearing* is a mere verbal question of little moment." " If the conclusions drawn from the experiments above described are legitimate, plasticity must be admitted by the side of sliding and fracture and regelation, as one of the constituent elements of the theory of glacier-motion, and a more important place in that theory must be assigned to the views of the late Principal Forbes than has for some years been conceded to them."

It was not, therefore, without some surprise that I read the following passages in the Canon's recent paper:—"Mr. Mathews, however, and M. Heim after him, as well as Mr. Ball, deny that glacier-ice shears at all. They say that it bends." " The idea present in common to the minds of these gentlemen seems to be that in bending so as to take a set, ice does not shear. In this I venture to think there is a misapprehension. In the bending of a plate of ice every particle, except those at the points of support, is made to move in the direction in which the plate is bent—those particles which are at the point of greatest inflection being made to move furthest, and those nearer to it being always made to move further than those more remote ; so that every particle moves over that which is alongside towards the nearest point of support; and being assumed to have taken a set, it must have sheared over it."

If the Canon will do me the favour to reperuse the paragraph from the Alpine Journal quoted in support of the language which he puts into my mouth, he will see that it has an opposite significance to that which he attaches to it. I have hesitated, it is true, to identify the molecular displacements of the ice-plank with the shearing described by Canon Moseley ; but I should be the last to deny that his account of the former phenomenon is a possible hypothesis. Let us by all means adopt it, for the sake of argument, and see to what it leads us.

The cross section of the ice-plank is 12 square inches. In order that a slice of the plank may move over the next alongside, it must be subjected to a pressure equal to twelve times the unit

of shear of ice, or, taking the latter at 75 lbs., to a pressure of 900 lbs. The problem is, how is this pressure to be generated out of the gravitation of an ice-plank the entire weight of which is less than 32 lbs. ? I am curious to see the mathematical reasoning by which the Canon will perform this feat of mechanical legerdemain. If he declines it, may I invite him to consider the inverse problem, and to calculate the shearing-force of ice from the behaviour of an ice-plank under strain ?

My own mathematical resources are not adequate to the task ; but I venture to submit the following as an approximate solution. If the whole weight of the plank were employed in shearing it, and in shearing it only along the supporting edges, two surfaces, each with an area of 12 square inches, would be sheared by a force of 32 lbs., or $1\frac{1}{3}$ lb. per square inch. But, of the whole weight of the plank, one portion is employed in exerting a pressure upon the bearers, a second in exerting a stress upon the upper half of the plank and a strain upon the lower, leaving only a third portion available for producing shear. And this third portion is exerted, not merely at the supporting edges, but in shearing successive slices of the plank continuously from end to end. The shearing-force therefore must be less, probably very considerably less, than $1\frac{1}{3}$ lb. per square inch.

In the Philosophical Magazine for January 1870 Canon Moseley has given us a shearing-force varying, in round numbers, from 119 to 98 lbs. per square inch. In a subsequent experiment, described in his recent paper, he has sheared ice with a force of 63·6 lbs. per square inch. Surely, by judicious management, he may make still further progress in the same direction. The whole of his experiments on shearing, as hitherto conducted, are irrelevant to the problem of glacier-motion. In an actual glacier the dislocated masses of which it is composed are subject to longitudinal strains strikingly similar to those of a suspended ice-plank. It is possible that under this condition the resistance to shearing may be largely decreased ; and this condition was excluded in Canon Moseley's experiments.

I have compared a glacier to a series of ice-planks, placed upon their edges across the channel of the glacier, and occasionally fracturing into transverse prisms. The Canon objects that a real glacier is much more like a single ice-plank, placed upon its face with its length parallel to the axis of the channel. I do not concur in this opinion ; and if he would take the trouble to walk up the Mer de Glace, from the Montanvert to the Col du Géant, I think he would see cause to modify it.

There are other points in Canon Moseley's paper which are open to exception ; and these I propose to make the subject of a future communication.

XLII. *On the Effect of small Variations of Temperature on Steel Magnets.* By J. E. H. GORDON *and* WILLIAM NEWALL, *lately Students in the Laboratories of King's College, London**.

THE Astronomer Royal, in speaking of the effect of tempe- rature on magnets (Treatise on Magnetism, p. 68), says:— " The ratio of change . . . . is very different with different mag- nets; it probably depends on the quality of the steel, possibly on the mode of magnetization; but on this point nothing is known with certainty." We have been for some time engaged in investigating this subject, and have arrived at a result which we venture to hope will not be considered altogether devoid of interest. Our plan of proceeding was as follows. We took five steel bars procured from different makers, each bar about 5 mil- limetres in diameter and 80 millims. in length. We observed,

1st. The fall in magnet power for 1° C. between 20° and 50°.

2nd. The specific gravity of each bar at two temperatures; and from this we deduced the expansion rate.

3rd. The chemical composition, *i. e.* the percentages of iron and of impurity.

To ascertain the variations in the magnetic power, we used a magnetometer of Mr. Becker's make. The scale is about 2 metres long; it is curved to a radius of 2 metres, and is placed at that distance from the suspended magnet. The scale is di- vided into millimetres, each of which corresponds to 51·5 seconds of arc. It is illuminated either by a lamp or by sunlight re- flected from a mirror. The magnetometer was firmly clamped down upon a table of brickwork. The scale stood on the boarded floor. Its three legs were firmly screwed to the floor; and one end was wedged against the wall. A curtain of black linen with a hole for the telescope to come through was suspended behind the scale so as to form a background. On the base of the magnetometer, near the opposite end to that at which the mirror is suspended, is screwed a plate of ivory about 12 cen- tims. square by 6 millims. thick. On this lies the box which contains the disturbing magnet. The ivory is to prevent the heat of the box expanding the base and altering the distances. The box (which is of brass) is about 85 millims. long, 50 high, and 100 broad as the instrument lies. At each end is a circular hole 20 millims. in diameter. In these holes are in- serted disks of cork with holes in them, in which the magnet is fixed. When the magnet is fixed in position the corks are coated with shellac varnish; and while it is still wet, slips of glass warmed over a spirit-lamp are placed over them. This makes a perfectly water-tight joint. In the top of the box are fixed

* Communicated by the Authors.

two small tubes for water, and one larger one for a thermometer. The connexions from the water-tubes to the supply and waste-pipes are made by means of india-rubber tubing, so that no jar can be transmitted to the box. On the base, at each end of the ivory plate, is fixed a brass upright with a horizontal set-screw. The ends of these screws press on the glass clips just over the ends of the magnet, and keep the box in its place. The water supply is conveniently arranged so that by turning one tap hot water is made to circulate through the box, and by turning another cold water is sent through.

The thermometer is by Casella, divided on the stem; and on its scale the division representing 1° C. is about 1 millim. long. In the earlier experiments the readings were taken at 20°, 25°, 30°, and to 50° C.; afterwards it was found more accurate and less troublesome to take readings alternately at 20° and 50° C. The bars were hardened by heating them to dull redness (*i. e.* about 530° to 560° C. (Daniel)) and plunging them into cold water. They were then magnetized by being placed in a helix belonging to an electromagnet containing 35 lbs. of insulated wire. The helix was excited by from 4 to 6 pint Grove's cells. After being magnetized, each bar was dipped alternately three or four times into basins of hot (about 80°) and cold water. This was to remove any magnetism which would be permanently lost by a rise of 20° to 50° C. The specific gravities were taken in the ordinary way; the balance, however, was not good enough to make it worth while to correct for the buoyancy of the air. The apparent specific gravities in water at 15°·5 C. (60° F.) and at 37°·7 C. (100° F.) were taken. They were multiplied by the specific gravity of water at those temperatures. These specific gravities were calculated from data given by Dalton (see Miller's 'Elements of Chemistry,' vol. i. p. 249, § 133). They were

$$\text{at } 15°·5 \text{ C.} \quad . \quad . \quad . \quad . \quad 0·9940$$
$$,, \; 37°·7 \text{ C.} \quad . \quad . \quad . \quad . \quad 0·9833$$

If there is any error in these values it will merely affect the value of our constant, not its constancy. The analysis was conducted as follows, the percentage of iron only being determined, and the amount of impurities inferred from it. A standard solution of permanganate was first prepared by dissolving about 2 grms. of the pure crystallized salt in 1 litre of distilled water, this solution being standardized in the ordinary manner by dissolving 0·5 grm. of pure iron in dilute sulphuric acid and testing with the permanganate solution, when it was found that 1 cubic centim. of the solution equalled 0·007573 grm. iron. Pieces were chipped off the magnets, carefully cleaned, dissolved in acid, and then tested with the solution, when the following results were obtained. The analysis is as follows :—

	Percentage of iron.	Mean.	Amount of impurity in 100,000 parts, = P.
Bar B	98·293 98·514 98·275 97·841 98·477	98·680	1320·0
Bar C	98·225 98·484 98·422 97·571	98·177	1823·0
Bar D	98·639 99·122 98·265	98·675	1325·0
Bar E	98·453 98·581 98·611 96·764 97·575 98·256	98·030	1970·0
Bar F .... N.B. These two just balance each other. When both are removed the mean is 99·125.	98·431 98·862 96·711 105·221 99·698 99·508	99·123	877·0

We will now give the results of the magnetic determinations. F = the fall for 1° C. expressed as a decimal of the deflection at 20° C.

Bar B.   Bar C.

Tempe-rature.	Deflection.		Fall in 30°.	F.	Tempe-rature.	Deflection.		Fall in 30°.	F.
20	558·5	......	} 12·2		50	......	296	} 12·0	
50	......	546			20	308	......		
20	558·0	......			50	......	296		
50	......	543·5	} 12·8	·000738466	20	308	......	} 12·5	·00134755
20	554·5	......			50	......	295·5		
50	......	540			20	308	......		
20	548	......	} 11·5		50	......	295·5	} 12·7	
50	......	534			20	308	......		
20	543	......			50	......	295		
50	......	530·7	} 12·4		20	307·5	......	} 12·5	
20	542	......			50	......	294		
50	......	528·5			20	305·5	......		

Mean deflection at 20° = 550·7. Mean fall = 12·2.	Mean deflection at 20° = 307·2. Mean fall = 12·42.

Bar D.   Bar E.

Tempe-rature.	Deflection.		Fall in 30°.	F.	Tempe-rature.	Deflection.		Fall in 30°.	F.
50	......	551·5	} 15·7		50	......	155	} 7·0	
20	566·6	......			20	162·5	......		
50	......	550			50	......	156		
20	565	......	} 15·7		20	163·0	......	} 8·3	
50	......	547·8		·001388609	50	......	155 2		·00155040
20	562	......			20	164·0	......		
50	......	546	} 15·5		50	......	156·0	} 7·7	
20	561	......			20	164·0	......		
50	......	545			50	......	156·5		
20	559·7	......	} 15·8		20	164	......	} 7·4	
50	......	513·5			50	......	156·7		
20	559	......			20	164·2	......		

Mean deflection at 20°=562·2.   Mean deflection at 20°=163·6.
Mean fall =15·67.   Mean fall =7·6.

Bar F.

Tempera-ture.	Deflection.		Fall in 30°.	F.
50	......	327·7	} 12 4	
20	338·5	......		
50	......	324·5		
20	359·2	......	} 14 6	
50	......	324·7		·001389101
20	339·4	......		
50	......	325	} 14·8	
20	339·5	......		
50	......	324·3		
20	339·6	......	} 14·8	
50	......	325·5		
20	341	......		

Mean deflection at 20°=339·5.   Mean fall =14·15.

For C the mean of four determinations of 1 F by the old
method (*i. e.* taking observations at 20°, 25°, 30°, &c.) gives
·001329933.   The mean of this and of the one given above is
·00133874·5.

Bar.	Specific gravity at 15°·5 C.		Specific gravity at 37°·7.		Decrease in specific gravity for 22°·2 C.	Specific gravity at 20°, calcula- ted, =G.	Fall in specific gravity per 1° C. as a de- cimal of specific gravity at 20°, =e.
	Observed.	Cor- rected.	Observed.	Cor- rected.			
B ...	7·845	7·798	7·882	7·750	·048	7·788	·000278
C ...	7·880	7·833	7·896	7·764	·069	7·819	·000397
D ...	7·657	7·611	7·685	7·556	·055	7·600	·000326
E ...	7·806	7·760	7·819	7·688	·072	7·745	·000419
F ...	7·883	7·836	7·899	7·767	·069	7·822	·000397

Bar.	G^3.	Decrease in magnetic power for 1° C. as a decimal of magnetic power at 20°, =F.	Amount of impu- rity in 100,000 parts, =P.	Log P.	$P \cdot 7$.	$\dfrac{FG^3}{e}$.	$\dfrac{FG^3}{eP \cdot 7}$.
B ...	471·48	·000738466	1320	3·12057	265·8	1252·4	4·712
C ...	478·05	·001338745	1823	3·26012	341·3	1612·0	4·723
D ...	438·98	·000928042	1325	3·12221	266·6	1249·6	4·687
E ...	464·53	·00155040	1970	3·29446	362·9	1718·8	4·736
F ...	478·64	·001389101	877	2·94300	212·1	1674·7	7·7

Mean of B C D E = 4·714.
Extreme error (D) = 0·57 per cent.

The difference in bar F is immense, but is, we think, accounted for by the fact that the bar is not homogeneous. Magnets cut from different pieces of it gave values of (F) varying from ·0013 to ·0020, while the specific gravity varied from 7·771 to 7·836. Without this, the want of agreement in the analysis is sufficient of itself to show that the bar is not homogeneous.

We think it most likely that the impurity, when heaped up at the poles or centre of the magnet, would have a very different effect from what it would have when evenly distributed.

To sum up, for any homogeneous steel bar hardened at dull redness, the law which seems to hold with some exactitude is :—

If F = the decrease in magnetic power for 1° C. expressed as a decimal of the magnet power at 20° C.,

G = the specific gravity at 20° C.,

e = the decrease of specific gravity for 1° C. expressed as . a decimal of specific gravity at 20° C.,

P = the number of parts of impurity in 100,000 parts of the steel (all that is not iron being called impurity),

then $\dfrac{FG^3}{eP^{\cdot 7}}$ = a constant, and the constant is

$$4{\cdot}714.$$

It may be worth while to mention that

$$\frac{1}{P^{\cdot 7}} = \frac{\sqrt[9]{P}\ \sqrt[27]{P}}{P\sqrt[9]{P}}\ \&c.$$

Regarding it in this form may throw* some light on its mode of action.

We particularly wish to make the reservation that the law holds only for homogeneous bars of the same hardness.

We hardened our bars by plunging them into water when they were at a red heat, just visible in daylight in the darkest corner of a room. When tempered there is a slight difference, though we cannot say in which direction. When hardened at a full red or approaching a white heat, the magnet power increases and the value of F diminishes considerably.

We hope to make some measurements of the effects of hardness on the magnetic power and the value of F the subject of a future paper.

When the effect of hardness on the value of F is determined, our constant, which we venture to name the " *Thermo-magnetic constant of steel,*" will merely have to be multiplied by a function of the hardness to make it of universal application.

Almost all the quantities concerned are absolute, the only measures referred to being the Centigrade scale, and the assumption that the specific gravity of water at 4° C. = 1·000.

We are only beginners in scientific work, and by no means wish our measurements to be received as absolutely correct; but we think we have shown such a tendency towards a law as may induce some older experimenter to make some determinations of the constant with a greater degree of accuracy than we have been able to attain.

August 21, 1871.

XLIII. *On the Theory of Exchanges.*
By Henry Hudson, *M.D.**

PREVOST'S celebrated doctrine, under the above title, was an "Emission Theory of Heat," and, having regard to the then existing state of knowledge, was undoubtedly a most ingenious mode of explaining the apparent "radiation of cold."

The objections to it, in connexion with Leslie's discovery of the influence of surface on radiation, have not, I think, been sufficiently attended to; and after briefly considering the matter theoretically, I shall adduce, as I believe, conclusive experimental evidence against it.

There can be no doubt that the ingenious speculations of Professor Balfour Stewart and others have contributed to the advancement of the doctrine of exchanges in no small degree, as being immediately connected with the well-known parallelism (or equality) of the "radiating and absorbing powers" of the same body for heat. But to my mind these qualities are essentially due to the "different capacities for heat" of the surfaces. Thus, if two bodies, in a common medium, are equally *above* (or *below*) the temperature of that medium, the tendency of each body to attain the temperature of the medium will be perfectly equal.

Each therefore tends to radiate (or absorb) the same (say $\frac{1}{N}$th) portion of its excess (or deficiency) of temperature, and consequently each body will radiate (or absorb) quantities of heat proportional to its "capacity for heat." Hence radiation and absorption (being both due to the same quality in bodies) must be proportional to each other.

Assuming a "wave-theory" of heat, let us suppose a body (A) placed in a medium of the same temperature as itself (*i. e.* both body and medium being in a state of perfect equilibrium as to molecular and ætherial vibrations). How is it possible to conceive that this body can lose any portion of its vibratory motion so long as that of the surrounding medium continues undisturbed? If a body (B), say of a lower temperature, be introduced into (or placed outside) the medium, the vibrations of A are, of course, liable to be diminished. But this effect (on a wave-theory as distinguished from "emissive power") can only occur through a disturbance in the medium extending from B to A, whereby the equilibrium which previously existed is subverted. In accordance with these views, it appears impossible to admit that a body can show any of the effects termed "radiation" so long as itself and its surrounding medium are in a perfect state of equilibrium.

vibration ; and hence the idea that the temperature of the earth's surface can fall (by radiation) below that of the atmosphere immediately resting on it (as is assumed in the received theory of dew-formation) I hold to be utterly untenable.   Again, if we imagine such a body, at any temperature, to exist *alone* in boundless space, it appears to follow (on Prevost's theory), whatever be the temperature of the medium, that the vibrations of the molecules of the body must ultimately cease altogether in consequence of its receiving no return for its own wasted radiation.

<center>*Experimental evidence.*</center>

Let A B represent a metallic mirror, in whose focus (C) one of the balls of a differential thermometer is placed, E a cubic canister (two of its adjacent sides being bright metal, the other two varnished), F representing a large tin screen.   Suppose E to be filled with ice-water, and its metallic surface facing the mirror. The focal ball will be *chilled*, by an apparent " radiation of cold " from the canister, which is reflected by the mirror to its focus. The admirers of Prevost say that E (being *colder* than A B) can no longer radiate so much heat to it as it received (and reflected to C) when E was of its own temperature, and therefore the temperature of C is reduced.   Well—let the varnished surface of E be turned towards the mirror, and we find as a result that C is *more* chilled than it had been by the metallic surface ; thus a more powerful " radiation of heat," which ought *at least* to *diminish* the previous chill, causes the reflection of more cold to the focal ball.   I have always considered the above simple experiment of Leslie's to be fatal to Prevost's theory of exchanges ; for the radiation (from either A B, or C) is assumed to be wholly independent of the radiation of E, and consequently we have only to consider the effect of the latter as a radiator.

There are many variations of Leslie's experiment, all leading to the same conclusion.   Thus, having gilt one of the balls of his differential thermometer, he denominated it a " pyroscope." Now, in repeating the above experiment with this instrument, the results correspond with those already related, whether the gilt or the glass ball be placed in the focus ; in *every* case the var-

nished canister produces the greater *chill* ; and it is obvious that what might appear a *plausible* explanation with one ball in the focus must be an utter failure in the case of the other focal ball. In fact, however, when canister, mirror, and thermometer are all at the same temperature as the air, it appears unaccountable (on Prevost's theory) that the "superior radiative energy" of the varnished canister does not heat the focal ball when concentrated upon it by the reflection of the mirror.

There is a very pretty experiment of Professor Tyndall's with a thermomultiplier, which (although I have never tried it) will, I doubt not, prove the same thing. A *warm* body at the distance of 4 feet 6 inches from the instrument shows scarcely any action on the pile (most of the rays being scattered in space); but on introducing a polished tin tube (4 feet long) between the pile and the source of heat, the rays are reflected from its sides, and thus, by several internal reflections, reach the pile and produce a decided motion of the needle. Only substitute (in this experiment) a *cold* canister as the radiator, and the *chilling* influence of the varnished side will doubtless show its superior efficiency.

Seeing that the scientific world generally admitted Prevost's theory, I brought forward at the first Dublin Meeting of the British Association (in 1835) what I considered an " experimentum crucis." I heated the surface of the mirror (by filling it with hot water) to about 170° Fahr., and (keeping one ball in the focus) shifted the differential thermometer until its liquid stood at zero, both balls being then equally heated by the mirror. The temperature of the air in the room was 55° Fahr. ; the cubic canister (containing water at 67°), being placed just in front of the screen F (see figure), acted as a radiator of heat, and the varnished side was most efficient. On moving the canister nearer to the mirror, the effects diminished and at length ceased, the thermometer remaining at zero whether the varnished or metallic side of the canister faced the mirror ; but on moving the canister still nearer to the mirror, it began to act as a cold body, and the varnished side produced the greater chill.

I trusted at the time that this experiment (supplementary to those of Leslie already referred to) would have satisfied scientific physicists that Prevost was in error and that there could be no phenomena of radiation so long as a body was of the same temperature with the medium in which it is placed ; and therefore I did not enter into the rationale of the experiment, which I believe to be as follows.

The heated mirror produces a gradation of temperature in front of itself (of course an unstable equilibrium) ; and when the canister is so situated that its surface and the medium in front

of it, next the mirror, are *in æquilibrio* as to temperature, there will be no radiation from either side of the canister; but, according as the latter is in a position of greater or less temperature than that of the medium, it will produce increased or diminished temperature of the focal ball, and in each case the varnished side will be most efficient.

This apparent " radiation of cold " I would call a " negative heat-wave." An analogy with waves in water may make my meaning clear. Thus, if at one end of a narrow channel of still water you immerse a conical-shaped vessel, a regular wave will be propagated to the other extremity of the channel, its surface at the origin of the wave being raised above the equilibrium level. This I call a positive wave. Again, after water-equilibrium has been reestablished, on withdrawing this cone from the water, another wave will be propagated to the further extremity of the channel; but now the surface at the origin of the wave will evidently be depressed below the equilibrium-level. This, therefore, I would call a negative wave, being a wave of exhaustion, like the wave of cold.

In conclusion, I have only to add that I believe all the phenomena of " absorption " (whether of light or heat) are to be explained on Young's beautiful theory of " interference of waves;" and, notwithstanding the experiments of Forbes, Melloni, and others in reference to the supposed polarization of heat, I confess to having grave doubts, being inclined to believe that the distinction between waves of light and of heat will be found to be due to the " ætherial vibrations " of the former being always in a plane perpendicular to the progress of the wave, whilst those of the latter take place in the direction of wave-propagation; thus these effects would be due to *different functions* of the ætherial vibrations. Hence the *proportionality* of light to heat is not to be expected; and it is easy to understand how a beam (containing both light- and heat-rays) may be sifted of *either* by transmission through certain substances without any sensible diminution of the other system of rays—a result apparently incomprehensible if there is no *essential* distinction between light and heat.

<div style="text-align:center">

XLIV. *On a New Steam-gauge.*
By Professor Ch. V. ZENGER*.

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THIS gauge is intended to avoid the defects of common air-gauges, which have hitherto prevented the employment of the air-manometer, and at the same time to be more accurate

and unalterable in its working than the spring gauges now commonly used for steam-boilers.

In the first place, it is a great defect in the common air-manometer that the divisions on the manometric tube diminish rapidly at high pressures, and consequently the reading becomes less and less accurate the higher the pressure. The new steam-gauge, on the contrary, possesses the same degree of accuracy at all pressures, and even enables us to make the accuracy of reading greater at high pressures.

Another serious defect of the air-manometer is the liability to rupture of the narrow column of mercury, consequent upon the sudden shutting off or turning on of the steam. This is entirely avoided in the present instrument by the use of two closed vessels communicating with each other only by very narrow capillary tubes. Finally, the small column of mercury enclosed in the glass tube of common air-manometers is subject to capillary depression, and to the disturbing effects of heat upon the air-bulb and upon the mercury. In the instrument now to be described, it is sought to avoid these defects by not using capillary tubes for the manometer, and by disposing the air and mercury in such a way as to make the effect of heat sensible.

The construction of the apparatus will be plainly seen from the annexed diagrams*, representing the apparatus in three different sections. The air-tube of the manometer consists of a series of tubes of equal lengths but different diameters, melted together before the lamp, and ending at the top in a glass bulb. The lower end is connected by an air-tight screw-joint with the first of two iron vessels containing mercury or some other liquid, and communicating with each other only by a very narrow capillary channel. The manometric tube is sealed at the bottom ; but there are two fine capillary openings through the side at points below the surface of the mercury or other liquid contained in the two iron vessels. Hence the communication of pressure from the steam or other compressed gas whose pressure is to be measured, and which presses directly upon the surface of the liquid in the second iron vessel, can only take place through a system of two successive capillary channels ; and the resistance which these channels oppose to the motion of the mercury by which they are filled makes it impossible for sudden changes to occur in the height of the manometric column, and thus entirely prevents the division of the column or the entry of steam or gas into the manometer.

The capacities of the globe and of the several tubes which compose the manometer are so adjusted that they decrease in the

* [For the loan of these diagrams we are indebted to the kindness of the Proprietors of ' The Engineer.'—Eds. *Phil. Mag.*]

same ratio as that in which the pressure increases, which is evidently what is required by Mariotte's law in order that an in-

SECTIONAL PLAN AT A.B
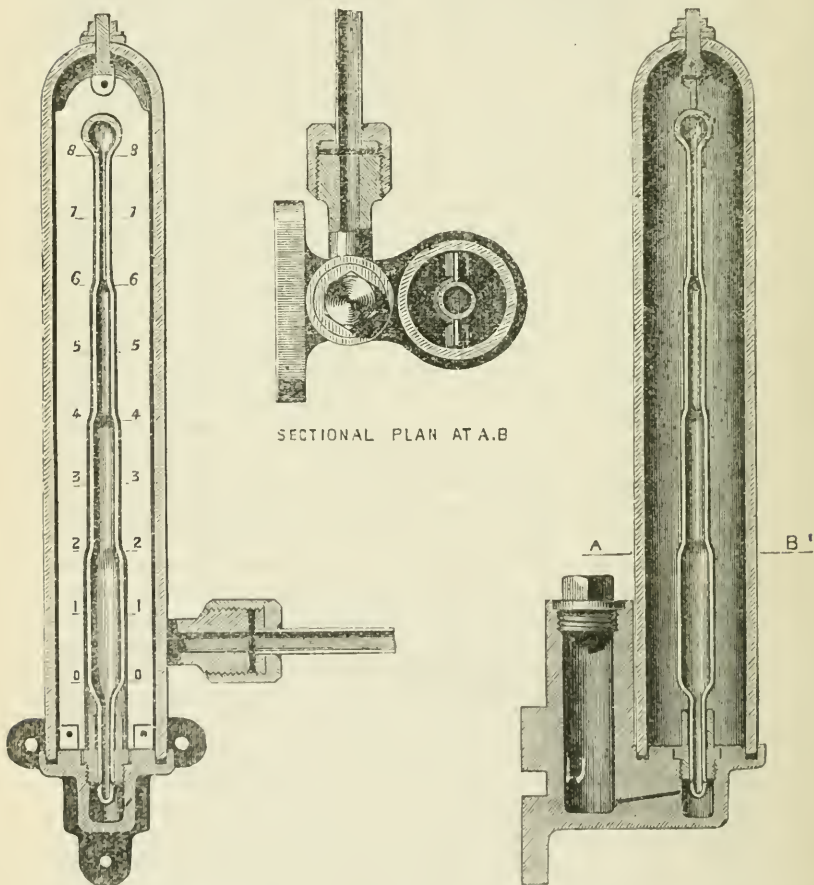
crease of pressure of *one* atmosphere may cause the first tube to be filled by the enclosing liquid, and that a further increase of pressure of the same amount may cause the second tube also to be filled, and so on, each equal increment of pressure causing the same rise of the liquid in the manometer-tube. This adjustment of the capacities is effected as follows :—Let the capacity of a manometer to be divided so as to show pressures up to, say, four atmospheres be called unity, and let r_1, r_2, r_3, and r_4 be the capacities of the first, second, and third tubes, and of the terminal globe respectively ; then we have

$v_1 + v_2 + v_3 + v_4 = 1 =$ volume occupied by the contained air under a pressure of *one* atmosphere ;

$v_2 + v_3 + v_4 = \frac{1}{2} =$ volume occupied by the air under a pressure of *two* atmospheres ;

$v_3 + v_4 = \frac{1}{3} =$ volume occupied by the air under a pressure of *three* atmospheres ;

$v_4 = \frac{1}{4} =$ volume of the air under *four* atmospheres.

This gives for the capacities of the separate tubes :

$$v_1 = \frac{1}{2} = \frac{1}{1 \cdot 2},$$

$$v_2 = \frac{1}{6} = \frac{1}{2 \cdot 3},$$

$$v_3 = \frac{1}{12} = \frac{1}{3 \cdot 4},$$

$$v_4 = \frac{1}{4}.$$

These equations indicate the general form of the expressions for the capacities of the separate tubes of a manometer to be divided so as to show pressures up to n atmospheres ; and from the capacities the radii of the tubes and terminal globe are at once obtained : thus—

Capacities.

$$v_1 = \frac{1}{1 \cdot 2},$$

$$v_2 = \frac{1}{2 \cdot 3},$$

$$v_3 = \frac{1}{3 \cdot 4},$$

$$\vdots \qquad \vdots$$

$$v_{n-1} = \frac{1}{(n-1)n},$$

$$v_n = \frac{1}{n}.$$

Radii.

$$r_1 = \frac{1}{\sqrt{2\pi h}}.$$

$$r_2 = \frac{1}{\sqrt{6\pi h}}.$$

$$r_3 = \frac{1}{\sqrt{12\pi h}},$$

$$\vdots \qquad \vdots$$

$$r_{n-1} = \frac{1}{\sqrt{(n-1)n\pi h}},$$

$$r_n = \frac{1}{\sqrt[3]{\frac{1}{3}n\pi}}.$$

If the length h of each tube is made equal to the unit of length, an inch for instance, it becomes unnecessary to express it in the above values of the radii.

In order to diminish the number of tubes required for the construction of the apparatus, it is sometimes convenient to arrange

the capacities so that the first is filled when the pressure in-
creases by two atmospheres, the second when it increases by four,
and so on. In this way three tubes suffice for a manometer that
is to show pressures up to 6 atmospheres, four tubes for one
that is to go up to 8 atmospheres, and so on. In this case the
capacities of the separate tubes are given by formulæ of the same
kind as before—namely, for a manometer to be used up to 6
atmospheres :—

$$v_1 + r_2 + r_3 + r_4 = 1, \qquad\qquad r_1 = \frac{2}{3} = \frac{2}{1 \cdot 3},$$

$$r_2 + r_3 + r_4 = \frac{1}{3}, \qquad\qquad r_2 = \frac{2}{15} = \frac{2}{3 \cdot 5},$$

$$r_3 + r_4 = \frac{1}{5}, \qquad\qquad r_3 = \frac{2}{35} = \frac{2}{5 \cdot 7},$$

$$r_4 = \frac{1}{7}. \qquad\qquad v_4 = \frac{1}{7}.$$

To prevent the accidental breakage of the manometer, it is
fastened to the graduated brass plate, and with it screwed to a
glass cover, $\frac{1}{4}$ of an inch thick, capable of supporting a pressure
of 20 atmospheres. The glass cover is pressed by a screw at
the top upon a plate of india-rubber, thus entirely preventing the
entrance of dust or damp.

The apparatus has already been at work for eighteen months
in several manufactories in Austria; and it has been found, by
comparative experiments with it on the Northern and State rail-
ways of Austria, that it is steadier and more accurate than the
standard spring and piston manometers used by the companies
for graduating their own manometers. Two of these instru-
ments were placed side by side on the same steam-boiler, with
a new spring manometer by MM. Bendenberg and Schaffer, of
Magdebourg; but up to the time of my leaving Prague (that is
to say, during $1\frac{1}{2}$ year) they remained in strict accordance all the
time, not $\frac{1}{2}$ pound difference of pressure being indicated by
them, although the temperature of the engine-room varied
greatly, and one of the air steam-gauges was placed directly on
the boiler (on direct steam), and quite near the fire, where the
temperature reached 30° or 35° C.

The elasticity of air not being subject to alterations like the
elasticity of a spring, which is greatly affected by heat and by
rapid variations of pressure, the new gauge is more steady and
requires less attention than spring gauges of any kind. It also
possesses a still greater advantage in being much more sensible
to small changes of pressure, and indicating them instanta-

neously, which is unattainable with spring manometers; for the air-gauge showed a diminution of pressure of about $\frac{1}{2}$ pound when the steam-whistle of the locomotive-boiler to which it was attached was sounded, whereas no motion could be detected in the spring and piston manometer.

The price of the new manometer being no higher than that of the spring manometer, but permitting greater certainty and accuracy in the reading, it is hoped that it may be found to be an accurate and durable gauge for steam-engines of every description, as well as for measuring the pressure of air in ventilation, and in the vacuum-pans used in sugar manufactories.

XLV. *On the steady Flow of a Liquid.* By HENRY MOSELEY, *M.A., D.C.L., Canon of Bristol, F.R.S., Corresponding Member of the Institute of France, &c.*

[Continued from p. 197.]

IT is by *experiment* that the value of γ or of $\dfrac{wi}{\mu}$ in the equation $\gamma = r_0 \epsilon^{-\gamma r}$ has been shown to be approximately constant. It results, as shown in Table I., from the approximate constancy in M. Darcy's experiments of the fractions $\left(\dfrac{v_1}{v_0}\right)$ and $\left(\dfrac{v^2 - v_0^2}{v^2 - v_1^2}\right)^{\frac{1}{2}}$, and from the near accordance with experiment of the values of v for different values of r calculated on the supposition of the constancy of γ. I propose now to investigate the value of γ theoretically.

The motion of the Liquid in the pipe is dependent on that in the reservoir.

A film of liquid flowing from a reservoir through a pipe may be considered as a cylindrical prolongation of a vase-like liquid film which begins in the reservoir, and which, contracting its external dimensions and increasing its thickness from the surface as it descends, bends its neck to pass into the pipe. The motion of the liquid which forms this vase is continuous. In the reservoir it is subject to the same kind of resistances as in the pipe; and the conditions of its motion in the pipe can only be fully determined when those in the reservoir shall have become known. U_2* represents the work expended on the various resistances opposed to the descent of the liquid in the reservoir and to its passage from the reservoir into the pipe; and this is one of the terms of equation (2) which are neglected in equation (7) and in equation (12). I am about to show that, by assigning a proper value to γ in each case, the error arising from this omission may be corrected. U_1 is another of the terms of equation

* Phil. Mag. September 1871, p. 186.

(2) which, although not neglected in equation (7), is neglected in equation (12). It represents the work carried away by the liquid which flows from the pipe in each unit of time. By the leaving out of these terms $U_1 + U_2$ from the right-hand member of equation (2), it becomes an *inequality*. To restore its equality, its left-hand member U must be diminished; that is to say, h must be assumed to be less than it really is. Let it become $\dfrac{h}{\gamma}$, where γ is an unknown function of U_1 and U_2 such that the substitution of $\dfrac{h}{\gamma}$ for h in the value of U will restore the equality which has been destroyed in equation (2) by omission of $U_1 + U_2$ and correct the error which has resulted therefrom in equation (12). The value of γ is shown by experiment (see Tables I., II., III.) to vary so slowly with the diameter of the pipe as to be nearly the same for diameters from 0·188 metre to 0·2447 metre. It becomes sensibly less, however, for diameters from 0·297 metre to 0·5 metre. For the former diameters, its value is about 2·25; for the latter, as shown by the following Table, the values of γ, 1·5 and 1, agree more nearly with experiment.

TABLE IV.

Index number.	By experiment.				By theory, $v = v_0 \epsilon^{-1\cdot5r}$.			
	Diameter = 0·297 metre.							
	r.				r.			
	0·1023.	0·102.	0·052.	0.	0·1023.	0·102.	0·052.	0.
	v_3.	v_2.	v_1.	v_0.	v_3.	v_2.	v_1.	v_0.
*	0·355	0·355	0·410	0·435	0·373	0·374	0·403	0·435
*	1·236	1·230	1·356	1·418	1·216	1·217	1·344	1·418
*	1·665	1·666	1·839	1·931	1·656	1·657	1·787	1·931
*	2·365	2·355	2·590	2·708	2·323	2·324	2·505	2·708

Index number.	By experiment.				By theory, $v = v_0 \epsilon^{-r}$.			
	Diameter = 0·5 metre.							
	r.				r.			
	0·1723.	0·170.	0·90.	0.	0·1723.	0·170.	0·090.	0.
	v_3.	v_2.	v_1.	v_0.	v_3.	v_2.	v_1.	v_0.
192.	0·475	0·477	0·535	0·571	0·481	0·482	0·522	0·571
194.	0·795	0·796	0·869	0·919	0·773	0·776	0·840	0·919
197.	1·1197	1·115	1·241	1·319	1·111	1·112	1·205	1·319

* No index number is given to these experiments in the work of M. Darcy. They are to be found at p. 143.

It is important to observe that the indeterminateness of the function γ is due not to our ignorance of the conditions of the motion of the liquid in the pipe, but of its antecedent motion in the reservoir. Whenever the conditions of the latter shall be expressed mathematically, γ will be determined and the solution of the problem will be complete. Meanwhile there is an important case in which the value of γ is necessarily *unity,* and in which the solution may be completed. It is that of an *open* inclined channel in which the liquid has attained a uniform depth and a steady equable motion. Before it has reached the point where this begins to be the case, the work U_1 has been already communicated to it, and the work U_2 has been already done by a pressure (that of the water in the reservoir) which no longer acts upon it. For the rest of its motion, all the work U that has to be done by its weight is the overcoming of the resistances to its motion in the channel; so that in equation (2) $U_1 + U_2 = 0$, and $U = U_3 + U_4$. I shall discuss this case in a subsequent and concluding paper.

The pressure on the liquid at any point in the pipe.

By a well-known formula[*] of the steady motion of a liquid, neglecting the consideration of those resistances whose unit is, in the case of a pipe, represented by μ,

$$p_1 = p' + wz - \frac{1}{2}\frac{w}{g}v^2 ;$$

where p_1 represents the unit of pressure on any molecule of the liquid whose velocity is v, p' that at the common surface of the liquid and the atmosphere, z the depth of the molecule below the surface, and w the weight of a cubic unit. Let this equation be supposed to obtain as long as the water is in the reservoir but not in the pipe, and let the depth of the aperture be $\frac{h}{\gamma}$. At the aperture it becomes

$$p_1 = p' + w\frac{h}{\gamma} - \frac{1}{2}\frac{w}{g}v^2. \quad . \quad . \quad . \quad . \quad (18)$$

If p be taken to represent the unit of pressure at a point whose distance from the entrance of the pipe is x, and from its axis r, and if a section be imagined perpendicular to the axis through that point, the pressures on the liquid between it and the entrance to the pipe will be in equilibrium.

---

[*] Treatise on Hydrostatics, by the author of this paper, p. 147. Cambridge: 1830.

$$\therefore \int_0^r (2\pi r dr)p + \int_0^r (2\pi r x \mu)dr = \int_0^r (2\pi r dr)p_1 ;$$

$$\therefore p + \mu x = p_1 = p' + w\frac{h}{\gamma} - \frac{1}{2}\frac{w}{g}v^2 \text{ (by equation 18)} ;$$

$$\therefore p = p' + w\frac{h}{\gamma} - \mu x - \frac{w}{2g}v^2. \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Also, equation (13), $v = v_0 \epsilon^{-\gamma r}$,

$$\therefore p = p' + w\frac{h}{\gamma} - \mu x - \frac{wv_0^2}{2g}\epsilon^{-2\gamma r}. \quad . \quad . \quad . \quad . \quad (20)$$

At the orifice of the pipe by which the water escapes, $p = p'$ and $x = l$;

$$\therefore p' = p' + \frac{wh}{\gamma} - \mu l - \frac{wv_0^2}{2g}\epsilon^{-2\gamma r},$$

or

$$0 = \frac{wh}{\gamma} - \mu l - \frac{wv_0^2}{2g} \cdot \epsilon^{-2\gamma r}. \quad . \quad . \quad . \quad (21)$$

And if we neglect v_0^2, which is equivalent to neglecting U_1,

$$\gamma = \frac{wh}{\mu l} = \frac{wi}{\mu},$$

which is the value assigned to γ in equation (13), where U_1 is also neglected. By equation (19) it appears that the pressure on a point at a given distance x from the entrance of the pipe is less as its distance r from the axis is greater.

This fact is illustrated in the following instructive experiment of Professor Ludwig, of Leipsic[*]. In the accompanying diagram A B is the section of a pipe filled with water which flows through it. C D E is a continuous glass tube whose straight part C D passes through the pipe in a direction perpendicular to its axis, enters it by stuffing-boxes at c, d, and is capable of being moved in the direction C D without leakage. a and b are small apertures in this tube. The pipe being filled with water, the tube also fills with it. But the water in the pipe being in motion and the aperture a nearer to the axis than b, the pressure at a is by equation (20) less than that on b. The water from the pipe therefore flows along the tube through b in

[*] I do not know whether this experiment has been published; it was communicated to me by my son H. N. Moseley, to whom it was shown by Professor Ludwig.

the direction D E C, as shown by the arrows in the tube and the air-bubble.

Fig. 1.
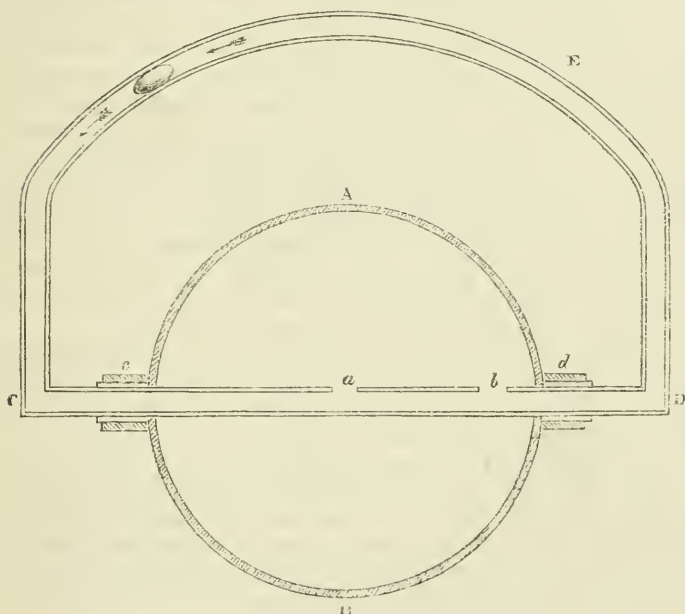
In all streams there cannot but be a tendency in the water to transfer itself from the sides, where the motion is slower and the pressure greater (as shown by equation 19), to the centre, where the motion is quicker and the pressure consequently less —and also to rise from the bottom to the surface, carrying up with it the mud.

The velocity of the central filament in a horizontal pipe.

The internal surface of the pipe resists the motion over it of the film of liquid which is in contact with it; that film resists the motion over it of the next film; that of the next, and so on to the central filament. Let A C D $c\,a$ (fig. 2) represent a lon-

Fig. 2.

gitudinal section, through the axis, of the liquid flowing uni-

formly through a circular pipe. The sections of the coaxial films are shown by the parallel lines in the figure, and the parts of these lines included in the space C D c represent those portions of the films which would have fallen off from the pipe in one unit of time if they had not moved on (as they are here supposed to do) in the same directions and with the same velocities after they had passed the end of the pipe as they had when they came to it.

Also let C B $b\,c$ be the space which on the same supposition would have been filled with liquid in one unit of time if there had been *no* resistance of the internal surface of the pipe, and therefore no differences in the velocities of the films; and, lastly, let A $d\,a$ be the space which would be left vacant in a unit of time at the upper end of the pipe if none were admitted there, and the surrounding liquid did not flow into the vacancy. The resistance of the internal surface of the pipe—

1st. *Stops back* in every unit of time as much of the liquid as would fill the space B C D $c\,b$, and therefore destroys as much of the *work* which would otherwise be done by the weight of the liquid in each unit of time as is represented by h multiplied by the weight of the liquid so stopped back.

2ndly. It causes the coaxial films of liquid to *slip* over one another, which otherwise they would not, and thus it *creates* the resistances of these in their motions over one another, and destroys as much of the work which would otherwise be done by the weight of the liquid effluent per unit of time as is represented by the aggregate *work* of those resistances.

Taking, therefore, U_3 to represent the work per unit of time of the resistance of the internal surface of the pipe, as before[*], and U_4 to represent the aggregate of the works per unit of time of the resistances of the films in moving over one another,

$$U_3 = (\text{weight of liquid which would fill the space B C D } c\,b)h + U_4.$$

But

$$(\text{weight B C D } c\,b)h = (\text{weight B C } c\,b)h - (\text{weight C } c \text{ D})h;$$

$$\therefore U_3 = (\text{weight B C } c\,b)h - (\text{weight C } c \text{ D})h + U_4.$$

But

$(\text{weight B C } c\,b)h =$ work which would be done by weight of efflux per unit of time if there were no resistance,

    ,,   ,, $= w\pi R^2 v$[†].

    [*] See Phil. Mag. September 1871, p. 186.
    [†] See Phil. Mag. September 1871, p. 188, equation (8).

Also

(weight $C \, c \, D$) h = work actually done by weight of efflux
per unit of time,

,,   ,, $= U *$;

$$\therefore U_3 = w\pi R^2 v h - U + U_4. \quad . \quad . \quad . \quad . \quad (22)$$

But

$$U_1 + U_2 + U_3 + U_4 = U \dagger;$$

$\therefore$ adding and striking out common terms,

$$U_1 + U_2 + 2U_3 = w\pi R^2 v h. \quad . \quad . \quad . \quad . \quad (23)$$

But

$$v = \sqrt{2gh} \ddagger;$$

by equation (5),

$$U_3 = 2\pi R l (\mu_1 + \lambda_1 V^2) V ;$$

by equation (4),

$$U_1 = \frac{\pi w}{g} \int_0^R v^3 r \, dr;$$

$$\therefore \frac{\pi w}{g} \int_0^R v^3 r \, dr + U_2 + 4\pi R l (\mu_1 + \lambda_1 V^2) V = w\pi R^2 h \sqrt{2gh}. \quad . \quad (24)$$

Now $v = \gamma_0 \epsilon^{-\gamma r}$,

$$\therefore U_1 = \frac{\pi w}{g} \int_0^R v^3 r \, dr = \frac{\pi w v_0^3}{g} \int_0^R \epsilon^{-3\gamma r} r \, dr = \frac{\pi w v_0^3}{9g\gamma^2} \left[1 - (1 + 3\gamma R)\epsilon^{-3\gamma R} \right].$$

But since $V = v_0 \epsilon^{-\gamma R}$, $\therefore v_0 = V \epsilon^{\gamma R}$,

$$\therefore U_1 = \frac{\pi w V^3}{9g\gamma^2} \left[\epsilon^{3\gamma R} - 3\gamma R - 1 \right] ; \quad . \quad . \quad . \quad (25)$$

$\therefore$ by equation (24),

$$\frac{\pi w V^3}{9g\gamma^2} \left[\epsilon^{3\gamma R} - 3\gamma R - 1 \right] + U_2 + 4\pi R l (\mu_1 + \lambda_1 V^2) V = w\pi R^2 h \sqrt{2gh}. \quad (26).$$

If μ_1 be exceedingly small as compared with $\lambda_1 V^2$, and U_2 be neglected,

$$\frac{\pi w V^3}{9g\gamma^2} \left[\epsilon^{3\gamma R} - 3\gamma R - 1 \right] + 4\pi R l \lambda_1 V^3 = \pi w \sqrt{2g} R^2 h^{\frac{3}{2}} ;$$

$$\therefore V = \frac{w^{\frac{1}{3}} (2g)^{\frac{1}{6}} R^{\frac{2}{3}} h^{\frac{1}{2}}}{\left[\frac{w}{9g\gamma^2} (\epsilon^{3\gamma R} - 3\gamma R - 1) + 4R l \lambda_1 \right]^{\frac{1}{3}}}. \quad . \quad (27)$$

* Phil. Mag. September 1871, p. 186.
† Ibid. p. 186, equation (2).
‡ Ibid. p. 188, equation (8).

But by equation (13),

$$v = v_0 \epsilon^{-\gamma r} \text{ and } V = v_0 \epsilon^{-\gamma R};$$

$$\therefore\ v = V \epsilon^{\gamma(R-r)};$$

$$\therefore\ v = \frac{w^{\frac{1}{3}}(2g)^{\frac{1}{6}} R^{\frac{2}{3}} h^{\frac{1}{2}} \epsilon^{\gamma(R-r)}}{\left[\dfrac{w}{9g\gamma^2}(\epsilon^{3\gamma R} - 3\gamma R - 1) + 4Rl\lambda_1\right]^{\frac{1}{3}}}; \quad . \quad (28)$$

$$\therefore\ r_0 = \frac{w^{\frac{1}{3}}(2g^{\frac{1}{6}} R^{\frac{2}{3}} h^{\frac{1}{2}} \epsilon^{\gamma R}}{\left[\dfrac{w}{9g\gamma^2}(\epsilon^{3\gamma R} - 3\gamma R - 1) + 4Rl\lambda_1\right]^{\frac{1}{3}}}. \quad . \quad (29)$$

The discharge per $1''$ *in cubic metres from a horizontal pipe.*
By equation (16),

$$Q = \frac{2\pi v_0}{\gamma^2}\left[1 - (1 + \gamma R)\epsilon^{-\gamma R}\right];$$

$$\therefore\ Q = \frac{2\pi(2g)^{\frac{1}{6}} w^{\frac{1}{3}} h^{\frac{1}{2}} R^{\frac{2}{3}}(\epsilon^{\gamma R} - \gamma R - 1)}{\gamma^2\left[\dfrac{w}{9g\gamma^2}(\epsilon^{3\gamma R} - 3\gamma R - 1) + 4Rl\lambda_1\right]^{\frac{1}{3}}}; \quad . \quad (30)$$

$$\therefore\ Q = \frac{\pi\sqrt{2g}(\epsilon^{\gamma R} - \gamma R - 1) R^{\frac{1}{3}} h^{\frac{1}{2}}}{\gamma^{\frac{4}{3}}\left[\dfrac{\epsilon^{3\gamma R} - 3\gamma R - 1}{36R} + \dfrac{l\gamma^2\lambda_1 g}{w}\right]^{\frac{1}{3}}}. \quad . \quad . \quad (31)$$

If, as before, we suppose the work with which the water finally leaves the pipe to have been already accumulated in it when it arrived at the third water-gauge in M. Darcy's experiments, and if h be taken to represent the difference of the heights of the first and third gauges, then, applying the above formula to M. Darcy's experiments, we must assume $U_1 = 0$, which is equivalent to neglecting the first term in the denominator of equation (30). In this case, therefore,

$$Q_1 = \frac{\pi\sqrt{2} \cdot w^{\frac{1}{3}} g^{\frac{1}{6}}(\epsilon^{\gamma R} - \gamma R - 1) R^{\frac{1}{3}} h^{\frac{1}{2}}}{\gamma^2 \lambda_1^{\frac{1}{3}} l^{\frac{1}{3}}}. \quad . \quad . \quad (32)$$

Substituting the numerical values of π, w, g and reducing,

$$Q_1 = 65 \cdot 004 \frac{(\epsilon^{\gamma R} - \gamma R - 1) R^{\frac{1}{3}} h^{\frac{1}{2}}}{\gamma^2 \lambda_1^{\frac{1}{3}} l^{\frac{1}{3}}}. \quad . \quad . \quad . \quad (33)$$

In the cases in which γ may be assumed $= 2 \cdot 25$,

$$Q_1 = 2 \cdot 7664 \frac{(\epsilon^{2 \cdot 25R} - 2 \cdot 25 R - 1) R^{\frac{1}{3}} h^{\frac{1}{2}}}{\lambda_1^{\frac{1}{3}} l^{\frac{1}{3}}} \quad . \quad . \quad (34)$$

It is the object of the following Tables to verify this formula by comparison with the experiments of M. Darcy.

In Table V., from the value of the discharge Q_1 given by experiment for a given head of water h in the case of any given pipe, the value of $\dfrac{Q_1}{h^{\frac{1}{2}}}$ is calculated. *That* is the value of the coefficient of $h^{\frac{1}{2}}$ in equation (34). Call it A. Then, if the formula be true,

$$Q_1 = A h^{\frac{1}{2}}$$

will represent the discharge *from the same pipe* for any head of water. This calculation is made in respect of three different pipes; and the results are compared with those given by experiment.

In Table VI., from the value of Q_1 (the discharge per $1''$ from a pipe of given radius R under a given head of water h) the value of $\dfrac{Q_1}{(\epsilon^{2\cdot 25\mathrm{R}} - 2\cdot 25\,\mathrm{R} - 1)\mathrm{R}^{\frac{1}{3}} h^{\frac{1}{2}}}$ is calculated. *That* is the value of the coefficient of $(\epsilon^{2\cdot 25\mathrm{R}} - 2\cdot 25\,\mathrm{R} - 1)\mathrm{R}^{\frac{1}{3}} h^{\frac{1}{2}}$ in equation (34). Call it B. Then, for all pipes of the same material and state of their internal surface and of the same length, whatever may be their diameters and heads of water, (if the formula be true) the experimental discharge ought to be represented approximately by the formula

$$Q_1 = B(\epsilon^{2\cdot 25\mathrm{R}} - 2\cdot 25\,\mathrm{R} - 1)\mathrm{R}^{\frac{1}{3}} h^{\frac{1}{2}}.$$

These calculations are made in respect of two new cast-iron pipes, whose radii were respectively ·0685 and ·094, and the results compared with experiment.

In Table VII., from given corresponding values of Q_1, R, h, l substituted in equation (34), the value of $\lambda_1^{\frac{1}{3}}$ is determined for pipes of different material and for different states of the surface of pipes of the same material. If two pipes of different diameters and under different heads of water be of the same material, and their internal surfaces in identically the same state, and if the joints of the separate lengths which compose the pipe be equally well (or ill) made (conditions exceedingly difficult to be realized), then the values of $\lambda_1^{\frac{1}{3}}$, as determined by the formula, ought (if it be true) to be nearly the same. They may be compared by reference to the Table. All the experiments of M. Darcy with pipes of the same material were made with the same *length* of pipe. It is impossible therefore by means of them to verify that condition of equation (4) by which (other things being the same) the discharge per $1''$ ought to vary inversely as $l^{\frac{1}{3}}$.

TABLE V.

New cast-iron pipe. Diameter 0·188 metre. $Q_1 = ·35636 h^{\frac{1}{2}}$.				Old cast-iron pipe with deposit. Diameter 0·2432 metre. $Q_1 = ·047462 h^{\frac{1}{2}}$.			
No.	h.	Q_1*. By experiment.	Q_1. By theory.	No.	h.	Q_1. By experiment.	Q_1. By theory.
	metres.	m. c.	m. c.		metres.	m. c.	m. c.
157.	·027	·005513	·005855	166.	·094	·0142724	·014551
158.	·175	·013786	·013788	167.	202	·020988	·021332
159.	·368	·021050	·021061	168.	·473	·032874	·032642
160.	·805	·03130	·03197	169.	1·150	·051371	·050897
161.	1·340	·04132	·04135	170.	2·290	·071823	·071823
162.	2·250	·05365	·05345	171.	3·200	·0885196	·084903
163.	3·810	·06956	·06956	172.	4·105	·096333	·096162
164.	10·980	·11953	·11809	173.	13 981	·1780833	·17747

The same old iron pipe, cleaned. Diameter 0·2477 metre.
$$Q_1 = ·062697 h^{\frac{1}{2}}.$$

No.	h.	Q_1. By experiment.	Q_1. By theory.
	metres.	metre.	metre.
174.	·052	·01433	·014297
175.	·165	·025225	·025468
177.	1·155	·065333	·067382
178.	2·035	·08944	·08944
179.	2·735	·10366	·10369
180.	3·730	·12089	·12109
181.	11·343	·21136	·21160

TABLE VI.

New cast-iron pipes.

$$Q_1 = 3·1981 (\epsilon^{2·25R} - 2·25R - 1) R^{\frac{1}{3}} h^{\frac{1}{2}}.$$

No.	h.	Diameter 0·137 metre. Q_1.		No.	h.	Diameter 0·188 metre. Q_1.	
		By experiment.	By theory.			By experiment.	By theory.
	metres.	m. c.	m. c.		metres.	m. c.	m. c.
147.	·024	·00220163	·002534	157.	·027	·00551315	·005752
148.	·087	·00440526	·004824	158.	·175	·013786	·014644
149.	·209	·00721812	·007477	159.	·368	·021055	·021235
150.	·475	·0112727	·011273	160.	·805	·0313032	·0314085
151.	1·26	·018898	·01836	161.	1·34	·04132	·040521
152.	2·25	·025321	·024535	162.	2·25	·053656	·052508
153.	3·318	·0309975	0297904	163.	3·81	·06956	·068328
154.	3·905	·03370	·032322	164.	10·98	·119861	·11559
155.	9·852	·053789	·05134	165.	14 591	·136787	·13371

* Darcy, *Recherches* &c., pp. 58, 59, 60.

Table VII.

$$\lambda_1^{\frac{1}{3}} = 2 \cdot 7664 \, (\epsilon^{2 \cdot 25 R} - 2 \cdot 25 R - 1) \frac{R^{\frac{1}{3}} h^{\frac{1}{2}}}{l^{\frac{1}{3}} Q_1}.$$

No.	h.	l.	R.	$(\epsilon^{2 \cdot 25R} - 2 \cdot 25R - 1)$.	Q_1. By experiment.	$\lambda^{\frac{1}{3}}$. By theory.	
150.	·475	100	·0685	·0125	·0112727	·18636	Cast iron, new.
160.	·805	100	·0940	·0241	·0313832	·18671	Cast iron, new.
111.	·670	100	·01795	·00082	·0002558	·40947	Cast iron with deposit.
123.	1·61	100	·03975	·00416	·0033627	·31928	Cast iron with deposit.
118.	1·441	100	·0182	·00085	·0006268	·25521	No. 111, cleaned.
130.	·737	100	·04005	·0042	·0031305	·23433	No. 123, cleaned. [men.
83.	15·605	100	·0413	·0045	·0209066	·176526	Iron covered with bitu-
94.	12·156	100	·098	·0251	·18143946	·13254	Iron covered with bitu-
57.	2·80	50	·001075	·001075	·0017384	·13178	Lead. [men.
59.	7·61	50	·0205	·001075	·0030401	·12423	Lead.
103	0·156	45	·02484	·00151	·00060699	·077926	Glass.
107.	5 020	45	·02484	·00151	·00410166	·10205	Glass.

The steady flow of a liquid in an inclined pipe.

In the experiments of M. Darcy on the flow of water through circular pipes, the pipes were laid horizontally; and the preceding investigation refers only to such pipes. It may readily, however, be adapted to the case of pipes upon a slope. If ι represent the uniform inclination of such a pipe into the top of which a liquid is poured so as to keep it full, and if l be the length of the pipe, then the vertical height through which the water has descended when it flows out of the pipe is represented by $l \sin \iota$, and the weight of the liquid which flows per unit of time through a film whose radius is r is represented by $2\pi w \int_0^r v r \, dr$. Therefore the work U done by the weight of this volume of liquid in descending the pipe is represented by the equation

$$U = 2\pi w l \sin \iota \int_0^r v r \, dr.$$

Substituting this value for U in equations (9) and (10) and proceeding as before,

$$\frac{v}{v_0} \left(\frac{v^2 - v^2}{v^2 - v^2} \right)^{\frac{1}{2}} = \epsilon^{-\frac{w \sin \iota}{\mu} r}, \quad \cdots \quad (35)$$

$$v^2 = \frac{v^2}{1 + \left[\left(\frac{v}{v_0} \right)^2 - 1 \right] \epsilon^{\frac{2w \sin \iota}{\mu} r}}. \quad \cdots \quad (36)$$

Or neglecting the work accumulated in the efflux, as before,

$$v = v_0 \epsilon^{-\frac{w \sin \iota}{2\mu} r}. \qquad \qquad (37)$$

Since $\dfrac{w \sin \iota}{2\mu} = \gamma$ is assumed to have the same value for all values of ι, R remaining the same, it follows that the ratio of the velocity of any film to that of the central filament is independent of the slope. Substituting $\dfrac{w \sin \iota}{2\mu}$ for γ in equations (16), (28), (31),

$$Q_R = 2\pi \left(\frac{2\mu}{w \sin \iota}\right)^2 \left[1 - \left(1 + \frac{w \sin \iota}{2\mu} R\right) \epsilon^{-\frac{w \sin \iota}{2\mu} R}\right] v_0, \quad (38)$$

$$v = \frac{w^{\frac{1}{3}} (2g)^{\frac{1}{6}} R^{\frac{2}{3}} h^{\frac{1}{2}} \epsilon^{\frac{w \sin \iota}{2\mu} (R-r)}}{\left[\dfrac{w}{9g}\left(\dfrac{2\mu}{w \sin \iota}\right)^2 \left(\epsilon^{\frac{3w \sin \iota}{2\mu}R} - \dfrac{3w \sin \iota}{2\mu} R - 1\right) + 4R\lambda_1\right]^{\frac{1}{3}}}, \quad (39)$$

$$Q = \frac{2(2g)^{\frac{1}{2}}\left(\epsilon^{\frac{w \sin \iota}{2\mu}R} - \dfrac{w \sin \iota}{2\mu}R - 1\right) R^{\frac{1}{3}} h^{\frac{1}{2}}}{\left(\dfrac{w \sin \iota}{2\mu}\right)^{\frac{4}{3}}\left[\left(\dfrac{\epsilon^{\frac{3w\sin\iota}{2\mu}R} - \dfrac{3w \sin \iota}{2\mu}R - 1}{36R}\right) + \dfrac{gl\lambda_1}{w}\left(\dfrac{w \sin \iota}{2\mu}\right)^2\right]^{\frac{1}{3}}}. \quad (40)$$

If, in equation (38), $R = \infty$ly,

$$Q_\infty = 2\pi\left(\frac{2\mu}{w \sin \iota}\right)^2 v_0. \qquad \qquad (41)$$

This may be considered the limit of the discharge of a pipe, of however great diameter, which is placed on a given slope, and whose central filament is made to move with a given velocity v_0.

The horse-power lost by water in flowing through a pipe.

The work lost by a liquid in flowing through a pipe is, 1st, that represented by U_3*, which is expended on the resistance to its flow by the internal surface of the pipe; and secondly, that, U_4*, expended internally on the resistances opposed to the flowing of the films over one another. The whole work so lost is therefore represented by $U_3 + U_4$. Now, by equation (5),

$$U_3 = 2\pi R l (\mu_1 + \lambda_1 V^2) V ;$$

or, neglecting μ_1 as small in comparison with $\lambda_1 V^2$,

$$U_3 = 2\pi l \lambda_1 R V^3.$$

* Phil. Mag. September 1871, p. 186.

Substituting for V its value from equation (27),

$$U_3 = \frac{2\pi(2g)^{\frac{1}{2}}w\lambda_1 l R^2 h^{\frac{3}{2}}}{\left(\dfrac{w}{9g\gamma^2}\right)\left(\epsilon^{3\gamma R} - 3\gamma R - 1\right) + 4l\lambda_1 R}. \quad . \quad (42)$$

Also, by equation (6),

$$U_4 = -2\pi l \int_0^R \mu r \left(\frac{dv}{dr}\right) dr.$$

But $\mu = \dfrac{wi}{\gamma}$; and (equation 13) $v = r_0 \epsilon^{-\gamma r}$;

$$\therefore \ U_4 = -2\pi l \int_0^R \frac{wi}{\gamma}(-\gamma r_0 \epsilon^{-\gamma r})dr = \frac{2\pi w h v_0}{\gamma}\int_0^R \gamma \epsilon^{-\gamma r} dr;$$

$$\therefore \ U_4 = \frac{2\pi w h v_0}{\gamma}(1 - \epsilon^{-\gamma R}).$$

Substituting for r_0 its value from equation (29),

$$U_4 = \left(\frac{2\pi w h}{\gamma}\right)\frac{w^{\frac{1}{3}}(2g)^{\frac{1}{6}}R^{\frac{2}{3}}h^{\frac{1}{2}}\epsilon^{\gamma R}(1 - \epsilon^{-\gamma R})}{\left[\dfrac{w}{9g\gamma^2}(\epsilon^{3\gamma R} - 3\gamma R - 1) + 4Rl\lambda_1\right]^{\frac{1}{3}}};$$

reducing,

$$U_4 = \frac{2\pi w^{\frac{4}{3}}(2g)^{\frac{1}{6}}R^{\frac{2}{3}}h^{\frac{3}{2}}(\epsilon^{\gamma R} - 1)}{\gamma^{\frac{1}{3}}\left[\dfrac{w}{9g}(\epsilon^{3\gamma R} - 3\gamma R - 1) + 4l\gamma^2\lambda_1\right]^{\frac{1}{3}}}. \quad . \quad . \quad (43)$$

The work lost per second in traversing the pipe being represented by $U_3 + U_4$, equations in which the unit of work is the French kilogram-metre, and 75 of these units per second being a horse-power, it follows that the

$$\text{horse-power lost} = \frac{U_3 + U_4}{75}. \quad . \quad . \quad (44)$$

The rise of temperature in water flowing through a pipe.

It results from the experiments of Mr. Joule, that every 424 French units of work or kilogram-metres which are expended on the friction of water raise the temperature of every kilogramme of it by 1° of the Centigrade thermometer. Now, if we suppose the motion of the water to have become steady, as also its temperature, and the material of the pipe to be perfectly non-conducting, so that no heat escapes from the water so long as it is in the pipe, then the whole work converted into heat per second is carried off by the water that escapes per second. Let $t°$ be the number of degrees Centigrade by which it raises the temperature of that water. Now Q cubic metres or 1000Q kilogrammes

escape per second; and 424 kilogram-metres being required to raise 1 kilog. of water 1° Cent., $424 \times t$ kilogram-metres are required to raise 1 kilog. by $t°$; it follows, therefore, that $424 \times t° \times 1000$ Q is the number of kilogram-metres necessary to raise the water which escapes per $1''$ by $t°$;

$$\therefore \ 424 \times t° \times 1000\,\mathrm{Q} = \mathrm{U}_3 + \mathrm{U}_4,$$

or

$$t° = \frac{\mathrm{U}_3 + \mathrm{U}_4}{424000\,\mathrm{Q}}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45)$$

in which expression Q, U_3, and U_4 are given by equations (31) (42), and (43).

[To be continued.]

XLVI. *Hydrokinetic Solutions and Observations.* *By* Sir WILLIAM THOMSON, *F.R.S.**

PART I. *On the Motion of Free Solids through a Liquid.*

THIS paper commences with the following extract from the author's private journal, of date January 6, 1858 :—

"Let $\mathfrak{F}, \mathfrak{Y}, \mathfrak{Z}, \mathfrak{L}, \mathfrak{M}, \mathfrak{N}$ be rectangular components of an impulsive force and an impulsive couple applied to a solid of invariable shape, with or without inertia of its own, in a perfect liquid, and let $u, v, w, \varpi, \rho, \sigma$ be the components of linear and angular velocity generated. Then, if the *vis viva*† (twice the mechanical value) of the whole motion be, as it cannot but be, given by the expression

$$\mathrm{Q} = [u, u]u^2 + [v, v]v^2 + \ldots + 2[v, u]vu + 2[w, u]wu + \ldots$$
$$+ 2[\varpi, u]\varpi u + \ldots,$$

where $= [u, u], [v, v]$, &c. denote 21 constant coefficients determinable by transcendental analysis from the form of the surface of the solid, probably involving only elliptic transcendentals when the surface is ellipsoidal : involving, of course, the moments of inertia of the solid itself : we must have

$$[u,u]u + [v,u]r + [w,u]w + [\varpi,u]\varpi + [\rho,u]\rho + [\sigma,u]\sigma = \mathfrak{F}, \&\mathrm{c}.,$$
$$[u,\varpi]u + [r,\varpi]v + [w,\varpi]w + [\varpi,\varpi]\varpi + [\rho,\varpi]\rho + [\sigma,\varpi]\sigma = \mathfrak{L}, \&\mathrm{c}.$$

† Henceforth T, instead of $\frac{1}{2}$Q, is used to denote the "mechanical value," or, as it is now called, the "kinetic energy" of the motion.

If now a continuous force X, Y, Z, and a continuous couple L, M, N, referred to axes fixed in the body, is applied, and if $\mathfrak{F}, \ldots$, &c. denote the impulsive force and couple capable of generating from rest the motion $u, v, w, \varpi, \rho, \sigma$, which exists in reality at any time t; or, merely mathematically, if $\mathfrak{F}$ &c. denote for brevity the preceding linear functions of the components of motion, the equations of motion are as follow :—

$$\left.\begin{array}{l} \dfrac{d\mathfrak{F}}{dt} - \mathfrak{D}\sigma + \mathfrak{Z}\rho = \mathrm{X}, \quad \dfrac{d\mathfrak{D}}{dt} = \&c., \\[2ex] \dfrac{d\mathfrak{L}}{dt} - \mathfrak{D}w + \mathfrak{Z}v - \mathfrak{M}\sigma + \mathfrak{N}\rho = \mathrm{L}, \\[2ex] \dfrac{d\mathfrak{M}}{dt} - \mathfrak{Z}u + \mathfrak{F}w - \mathfrak{N}\varpi + \mathfrak{L}\sigma = \mathrm{M}, \\[2ex] \dfrac{d\mathfrak{N}}{dt} - \mathfrak{F}v + \mathfrak{D}u - \mathfrak{L}\rho + \mathfrak{M}\varpi = \mathrm{N}. \end{array}\right\} \quad . \quad . \quad . \quad (1)$$

Three first integrals, when

$$\mathrm{X}=0, \quad \mathrm{Y}=0, \quad \mathrm{Z}=0, \quad \mathrm{L}=0, \quad \mathrm{M}=0, \quad \mathrm{N}=0, \quad . \quad (2)$$

must of course be, and obviously are,

$$\mathfrak{F}^2 + \mathfrak{D}^2 + \mathfrak{Z}^2 = \text{const.} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

resultant momentum constant ;

$$\mathfrak{L}\mathfrak{F} + \mathfrak{M}\mathfrak{D} + \mathfrak{N}\mathfrak{Z} = \text{const.} \quad . \quad . \quad . \quad . \quad (4)$$

resultant of moment of momentum constant ; and

$$u\mathfrak{F} + v\mathfrak{D} + w\mathfrak{Z} + \varpi\mathfrak{L} + \rho\mathfrak{M} + \sigma\mathfrak{N} = \mathrm{Q}." \quad . \quad . \quad (5)$$

These equations were communicated in a letter to Professor Stokes, of date (probably January) 1858, and they were referred to by Professor Rankine, in his first paper on Stream-lines, communicated to the Royal Society of London[*], July 1863.

They are now communicated to the Royal Society of Edinburgh, and the following proof is added :—

Let P be any point fixed relatively to the body; and at time t, let its coordinates relatively to axes OX, OY, OZ, fixed in space, be x, y, z. Let PA, PB, PC be three rectangular axes fixed re-

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[*] These equations will be very conveniently called the Eulerian equations of the motion. They correspond precisely to Euler's equations for the rotation of a rigid body, and include them as a particular case. As Euler seems to have been the first to give equations of motion in terms of coordinate components of velocity and force referred to lines fixed relatively to the moving body, it will be not only convenient, but just, to designate as " Eulerian equations " any equations of motion in which the lines of reference, whether for position, or velocity, or moment of momentum, or force, or couple, move with the body, or the bodies, whose motion is the subject.

latively to the body, and $(A, X), (A, Y), \ldots$ the cosines of the nine inclinations of these axes to the fixed axes OX, OY, OZ.

Let the components of the "impulse"* or generalized momentum parallel to the fixed axes be ξ, η, ζ, and its moments round the same axes λ, μ, ν; so that if X, Y, Z be components of force acting on the solid, in line through P, and L, M, N components of couple, we have

$$\left.\begin{array}{l} \dfrac{d\xi}{dt} = X, \quad \dfrac{d\eta}{dt} = Y, \quad \dfrac{d\zeta}{dt} = Z, \\[2mm] \dfrac{d\lambda}{dt} = L + Zy - Yz, \quad \dfrac{d\mu}{dt} = M + Xz - Zx, \quad \dfrac{d\nu}{dt} = N + Yx - Xy. \end{array}\right\} \quad (6)$$

Let $\mathfrak{F}, \mathfrak{D}, \mathfrak{Z}$ and $\mathfrak{L}, \mathfrak{M}, \mathfrak{N}$ be the components and moments of the impulse relatively to the axes PA, PB, PC moving with the body. We have

$$\left.\begin{array}{l} \xi = \mathfrak{F}(A, X) + \mathfrak{D}(B, X) + \mathfrak{Z}(C, X), \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \lambda = \mathfrak{L}(A, X) + \mathfrak{M}(B, X) + \mathfrak{N}(C, X) + \mathfrak{Z}y - \mathfrak{D}z, \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{array}\right\} \quad (7)$$

Now let the fixed axes OX, OY, OZ be chosen coincident with the position at time t of the moving axes PA, PB, PC: we shall consequently have

$$\left.\begin{array}{l} x = 0, \quad y = 0, \quad z = 0, \\[1mm] \dfrac{dx}{dt} = u, \quad \dfrac{dy}{dt} = v, \quad \dfrac{dz}{dt} = w, \end{array}\right\} \quad \ldots \quad (8)$$

$$\left.\begin{array}{l} (A, X) = (B, Y) = (C, Z) = 1, \\ (A, Y) = (A, Z) = (B, X) = (B, Z) = (C, X) = (C, Y) = 0, \\ \dfrac{d(A, Y)}{dt} = \sigma, \quad \dfrac{d(B, X)}{dt} = -\sigma, \quad \dfrac{d(C, X)}{dt} = \rho, \\ \dfrac{d(A, Z)}{dt} = -\rho, \quad \dfrac{d(B, Z)}{dt} = \varpi, \quad \dfrac{d(C, Y)}{dt} = -\varpi. \end{array}\right\} \quad (9)$$

Using (7), (8), and (9) in (6), we find (1).

One chief object of this investigation was to illustrate dynamical effects of helicoidal property (that is, right or left-handed asymmetry). The case of complete isotropy, with helicoidal quality, is that in which the coefficients in the quadratic expression for T fulfil the following conditions.

* See "Vortex Motion," § 6, Trans. Roy. Soc. Edin. (1868).

$$[u, u] = [v, v] = [w, w] \quad \text{(let } m \text{ be their common value)},$$
$$[\varpi, \varpi] = [\rho, \rho] = [\sigma, \sigma] \quad \text{,, } n \quad \text{,,} \quad \text{,,} \quad \text{,,}$$
$$[u, \varpi] = [v, \rho] = [w, \sigma] \quad \text{,, } h \quad \text{,,} \quad \text{,,} \quad \text{,,}$$
$$[v, w] = [w, u] = [u, v] = 0; \quad [\rho, \sigma] = [\sigma, \varpi] = [\varpi, \rho] = 0,$$

and

$$[u, \rho] = [u, \sigma] = [v, \sigma] = [v, \varpi] = [w, \varpi] = [w, \rho] = 0; \tag{10}$$

so that the formula for T is

$$\mathrm{T} = \tfrac{1}{2} \{ m(u^2 + v^2 + w^2) + n(\varpi^2 + \rho^2 + \sigma^2) + 2h(u\varpi + v\rho + w\sigma) \} \tag{11}$$

For this case, therefore, the Eulerian equations (1) become

$$\frac{d(mu + h\varpi)}{dt} - m(v\sigma - w\rho) = \mathrm{X}, \&c.,$$

and

$$\frac{d(n\varpi + hu)}{dt} = \mathrm{L}, \&c. \tag{11}$$

[Memorandum :—Lines of reference fixed relatively to the body.]

But inasmuch as (11) remains unchanged when the lines of reference are altered to any other three lines at right angles to one another through P, it is easily shown directly from (6), (7), and (9) that if, altering the notation, we take u, v, w to denote the components of the velocity of P parallel to three fixed rectangular lines, and ϖ, ρ, σ the components of the body's angular velocity round these lines, we have

$$\frac{d(mu + h\varpi)}{dt} = \mathrm{X}, \&c.,$$

and

$$\frac{d(n\varpi + hu)}{dt} + h(\sigma v - \rho w) = \mathrm{L}, \&c. \tag{12}$$

[Memorandum :—Lines of reference fixed in space],

which are more convenient than the Eulerian equations.

The integration of these equations, when neither force nor couple acts on the body (X = 0 &c., L = 0 &c.), presents no difficulty; but its result is readily seen from § 21 ("Vortex Motion") to be that, when the impulse is both translatory and rotational, the point P, round which the body is isotropic, moves uniformly in a circle or spiral so as to keep at a constant distance from the "axis of the impulse," and that the components of angular velocity round the three fixed rectangular axes are constant.

An isotropic heliçoid may be made by attaching projecting vanes to the surface of a globe in proper positions; for instance, cutting at 45° each, at the middles of the twelve quadrants of any three great circles dividing the globe into eight quadrantal triangles. By making the globe and the vanes of light paper, a body is obtained rigid enough and light enough to illustrate by its motions through air the motions of an isotropic heliçoid through an incompressible liquid. But curious phenomena, not deducible from the present investigation, will, no doubt, on account of viscosity, be observed.

Part II.

Still considering only one moveable rigid body, infinitely remote from disturbance of other rigid bodies, fixed or moveable, let there be an aperture or apertures through it, and let there be irrotational circulation or circulations (§ 60, "Vortex Motion") through them. Let ξ, η, ζ be the components of the "impulse" at time t, parallel to three fixed axes, and λ, μ, ν its moments round these axes, as above, with all notation the same, we still have (§ 26, "Vortex Motion")

$$\left.\begin{array}{l} \dfrac{d\xi}{dt} = \mathrm{X}, \text{ \&c.,} \\[2mm] \dfrac{d\lambda}{dt} = \mathrm{L} + \mathrm{Z}y - \mathrm{Y}z, \text{ \&c.} \end{array}\right\} \quad . \quad (6) \text{ (repeated).}$$

But, instead of for T a quadratic function of the components of velocity as before, we now have

$$\mathrm{T} = \mathrm{E} + \tfrac{1}{2}\{[u, u]u^2 + \ldots + 2[u, v]uv + \ldots\}, \quad . \quad (13)$$

where E is the kinetic energy of the fluid motion when the solid is at rest, and $\tfrac{1}{2}\{[u, u]u^2 + \ldots\}$ is the same quadratic as before. The coefficients $[u, u]$, $[u, v]$, &c. are determinable by a transcendental analysis, of which the character is not at all influenced by the circumstance of there being apertures in the solid. And instead of $\xi = \dfrac{d\mathrm{T}}{du}$, &c., as above, we now have

$$\left.\begin{array}{l} \xi = \dfrac{d\mathrm{T}}{du} + \mathrm{I}l, \quad \eta = \dfrac{d\mathrm{T}}{dv} + \mathrm{I}m, \quad \zeta = \dfrac{d\mathrm{T}}{dw} + \mathrm{I}n, \\[2mm] \lambda = \dfrac{d\mathrm{T}}{d\varpi} + \mathrm{I}(ny - mz) + \mathrm{G}l, \quad \mu = \text{\&c.,} \quad \nu = \text{\&c.,} \end{array}\right\} \quad . \quad (14)$$

where I denotes the resultant "impulse" of the cyclic motion when the solid is at rest, l, m, n its direction-cosines, G its "rotational moment" (§ 6, "Vortex Motion"), and x, y, z the coordinates of any point in its "resultant axis." These, (14)

with (13), used in (6) give the equations of the solid's motion referred to fixed rectangular axes. They have the inconvenience of the coefficients $[u, u]$, $[u, v]$, &c. being functions of the angular coordinates of the solid. The Eulerian equations (free from this inconvenience) are readily found on precisely the same plan as that adopted above for the old case of no cyclic motion in the fluid.

The formulæ for the case in which the ring is circular, has no rotation round its axis, and is not acted on by applied forces, though, of course, easily deduced from the general equations (14), (13), (6), are more readily got by direct application of first principles. Let P be such a point in the axis of the ring, and $\mathbb{C}$, A, B such constants that $\frac{1}{2}(\mathbb{C}\omega^2 + Au^2 + Bv^2)$ is the kinetic energy due to rotational velocity ω round D, any diameter through P, and translational velocities u along the axis and v perpendicular to it. The impulse of this motion, together with the supposed cyclic motion, is therefore compounded of

momentum in lines through P $\begin{cases} Au + I \text{ along the axis,} \\ Bv \text{ perpendicular to axis,} \end{cases}$

and moment of momentum $\mathbb{C}\omega$ round the diameter D.

Hence if OX be the axis of resultant momentum, (x, y) the coordinates of P relatively to fixed axes OX, OY; θ the inclination of the axis of the ring to O; and ξ the constant value of the resultant momentum, we have

$$\left. \begin{array}{l} \xi \cos \theta = Au + I; \quad -\xi \sin \theta = Bv; \quad \xi y = \mathbb{C}\omega; \\ \text{and} \\ \dot{x} = u \cos \theta - v \sin \theta; \quad \dot{y} = u \sin \theta + v \cos \theta; \quad \dot{\theta} = \omega. \end{array} \right\} \ . (15)$$

Hence for θ we have the differential equation

$$A\mathbb{C}\frac{d^2\theta}{dt^2} + \xi\left[I \sin \theta + \frac{A-B}{2B} \xi \sin 2\theta \right] = 0, \quad . \quad (16)$$

which shows that the ring oscillates rotationally according to the law of a horizontal magnetic needle carrying a bar of soft iron rigidly attached to it parallel to its magnetic axis.

When θ is and remains infinitely small, $\dot{\theta}$, y, and $\dot{y}$ are each infinitely small, $\dot{x}$ remains infinitely nearly constant, and the ring experiences an oscillatory motion in period

$$2\pi\sqrt{\frac{B\mathbb{C}}{[I + (A-B)\dot{x}](I + A\dot{x})}},$$

compounded of translation along OY and rotation round the diameter D. This result is curiously comparable with the well-known gyroscopic vibrations.

PART III. *The Influence of Wind on Waves in water supposed frictionless.* (*Letter to* Professor Tait, *of date August* 16, 1871.)

Taking OX vertically downwards and OY horizontal, let

$$x = h \sin n(y - \alpha t) \quad . \quad . \quad . \quad . \quad . \quad (1)$$

be the equation of the section of the water by a plane perpendicular to the wave-ridges; and let h (the half wave-height) be infinitely small in comparison with $\frac{2\pi}{n}$ (the wave-length). The x-component of the velocity of the water at the surface is then

$$-n\alpha h \cos n(y - \alpha t); \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and this (because h is infinitesimal) must be the value of $\frac{d\phi}{dy}$ for the point $(0, y)$, if ϕ denote the velocity-potential at any point (x, y) of the water. Now because

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0,$$

and ϕ is a periodic function of y, and a function of x which becomes zero when $x = \infty$, it must be of the form

$$P \cos (ny - e)\epsilon^{-nx},$$

where P and e are independent of x and y. Hence, taking $\frac{d\phi}{dx}$, putting $x = 0$ in it, and equating it to (2), we have

$$-Pn \cos (ny - e) = -n\alpha h \cos (ny - n\alpha t);$$

and therefore $P = \alpha h$, and $e = n\alpha t$; so that we have

$$\phi = \alpha h \epsilon^{-nx} \cos n (y - \alpha t). \quad . \quad . \quad . \quad (3)$$

This, it is to be remarked, results simply from the assumptions that the water is frictionless, that it has been at rest, and that its surface is moving in the manner specified by (1).

If the air were a frictionless liquid moving irrotationally, with a constant velocity V at heights above the water (that is to say, values of $-x$) considerably exceeding the wave-length, its velocity potential ψ, found on the same principle, would be

$$(V - \alpha) h\epsilon^{nx} \cos n (y - \alpha t) + Vy. \quad . \quad . \quad (4)$$

Let now q denote the resultant velocity at any point (x, y) of the air. Neglecting infinitesimals of the order $(nh)^2$, we have

$$\tfrac{1}{2}q^2 = \tfrac{1}{2}V^2 - V(V - \alpha)nh\epsilon^{nx} \sin n(y - \alpha t). \quad . \quad . \quad (5)$$

Now, if p denote the pressure at any point (x, y) in the air, and σ the density of the air, we have by the general equation for

pressure in an irrotationally moving fluid,

$$C - p = \sigma\left(\frac{d\psi}{dt} + \tfrac{1}{2}q^2 - gx\right). \quad . \quad . \quad . \quad . \quad (6)$$

Using (4) and (5) in this and putting $C = \tfrac{1}{2}\sigma V^2$, we find

$$-p = \sigma\left\{-nh(V-\alpha)^2\epsilon^{nx}\sin n(y-\alpha t) - gx\right\}. \quad . \quad (7)$$

Similarly if p' denote the pressure at any point (x, y) of the water, since in this case q^2 is infinitesimal, we have

$$-p' = nh\alpha^2\epsilon^{-nx}\sin n(y-\alpha t) - gx, \quad . \quad . \quad (8)$$

the density of the water being taken as unity.

Now let T be the cohesive tension of the separating surface of air and water. The curvature of the surface at any point (x, y) given by equation (1), being $\dfrac{d^2x}{dy^2}$, is equal to

$$-n^2h\sin n(x-\alpha t). \quad . \quad . \quad . \quad . \quad (9)$$

Hence at any point (x, y) fulfilling (1),

$$p - p' = Tn^2h\sin n(x-\alpha t); \quad . \quad . \quad . \quad (10)$$

and by (7) and (8), with for x its value by (1) (which, as h is infinitesimal, only affects their last terms), we have

$$p - p' = h\left\{n[\sigma(V-\alpha)^2 + \alpha^2] - g(1-\sigma)\right\}\sin n(x-\alpha t). \quad . \quad (11)$$

This, compared with (10), gives

$$n[\sigma(V-\alpha)^2 + \alpha^2] - g(1-\sigma) = Tn^2. \quad . \quad . \quad (12)$$

Let

$$w = \sqrt{\frac{g(1-\sigma) + Tn^2}{(1+\sigma)n}}, \quad . \quad . \quad . \quad . \quad (13)$$

which (being the value of α for $V = 0$) is the velocity of propagation of waves with no wind, when the wave-length is $\dfrac{2\pi}{n}$. Then (12) becomes

$$\frac{\alpha^2 + \sigma(V-\alpha)^2}{1+\sigma} = w^2, \quad . \quad . \quad . \quad . \quad (14)$$

which determines α, the velocity of the same waves when there is wind, of velocity V, in the direction of propagation of the waves. Solving the quadratic, we have

$$\alpha = \frac{\sigma V}{1+\sigma} \pm \left\{w^2 - \frac{\sigma V^2}{(1+\sigma)^2}\right\}^{\frac{1}{2}}. \quad . \quad . \quad (15)$$

This result leads to the following conclusions:—

(1) When

$$V < w \sqrt{\frac{1+\sigma}{\sigma}},$$

the values of α are positive and negative; that is to say, waves can travel with or against the wind. The positive value is always greater; that is to say, waves travel faster with than against the wind. The velocity of waves travelling *against* the wind is always less than w, the velocity without wind.

(2) When

$$V < 2w,$$

the velocity of waves travelling with the wind is greater than w. When

$$V = 2w,$$

the velocity of the waves *with* the wind is undisturbed by the wind; a result obvious without analysis. When

$$V > 2w,$$

the velocity of waves travelling with the wind is less than the velocity of the same waves without wind.

(3) When

$$V > w \sqrt{\frac{1+\sigma}{\sigma}},$$

waves of such length that w would be their velocity without wind, cannot travel against the wind.

(4) When

$$V > w \frac{1+\sigma}{\sqrt{\sigma}},$$

there cannot be waves of so small length as that for which the undisturbed velocity is w, and the equilibrium of the water is essentially unstable. And (13) shows that the minimum value of w is

$$\sqrt{\frac{2\sqrt{g\mathrm{T}(1-\sigma)}}{1+\sigma}}. \quad . \quad . \quad . \quad . \quad (16)$$

Hence the water with a plane level surface is unstable if the velocity of the wind exceeds

$$\sqrt{\frac{2\sqrt{g\mathrm{T}(1-\sigma^2)}}{\sigma}}.$$

W. T.

PART IV. (*Letter to* Professor Tait, *of date August* 23, 1871.)

Defining a ripple as any wave on water whose length $< 2\pi \sqrt{\frac{\mathrm{T}'}{g'}}$*, where

* Which for pure water $= 1 \cdot 7$ centim. (see Part V.).

$$g' = g\,\frac{1-\sigma}{1+\sigma}\,,$$

and

$$T' = \frac{T}{1+\sigma}$$

$$\left.\begin{array}{c}\\ \\ \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (17)$$

($\sigma = \cdot 00122$), you always see an exquisite pattern of ripples in front of any solid cutting the surface of water and moving horizontally at any speed, fast or slow. The ripple-length is the smaller root of the equation

$$\frac{2\pi}{\lambda}\,T' + \frac{\lambda}{2\pi}\,g' = w^2, \quad . \quad . \quad . \quad . \quad (18)$$

where w is the velocity of the solid. The latter may be a sailing-vessel or a row-boat, a pole held vertically and carried horizontally, an ivory pencil-case, a penknife-blade either edge or flat side foremost, or (best) a fishing-line kept approximately vertical by a lead weight hanging down below water, while carried along at about half a mile per hour by a becalmed vessel. The fishing-line shows both roots admirably; ripples in front, and waves of same velocity (λ the greater root of same equation) in rear. If so fortunate as to be becalmed again, I shall try to get a drawing of the whole pattern, showing the transition at the sides from ripples to waves. When the speed with which the fishing-line is dragged is diminished towards the critical velocity

$$\sqrt{2\sqrt{g'T'}},$$

which is the minimum velocity of a wave, being [see Part V. below] for pure water 23 centims. per second (or $\frac{1}{2\cdot 29}$ of a nautical mile per hour), the ripples in front elongate and become less curved, and the waves in rear become shorter, till at the critical velocity waves and ripples seem nearly equal, and with ridges nearly in straight lines perpendicular to the line of motion. (This is observation.) It seems that the critical velocity may be determined with some accuracy by experiment thus [see Part V. below]:—

Remark that the shorter the ripple-length the greater is the velocity of propagation, and that the moving force of the ripple-motion is partly gravity, but chiefly cohesion; and with very short ripple-length it is almost altogether cohesion, *i. e.* the same force as that which makes a dew drop tremble. The least velocity of frictionless air that can raise a ripple on rigorously quiescent frictionless water is [(16) above]

(being $\dfrac{1+\sigma}{\sqrt{\sigma}}$ × minimum wave-velocity)

660 centimetres per second

$$= 12 \cdot 8 \text{ nautical miles per hour.}$$

Observation shows the sea to be ruffled by wind of a much smaller velocity than this. Such ruffling, therefore, is due to viscosity of the air.

W. T.

Postscript to Part IV. (*October* 17, 1871).

The influence of viscosity gives rise to a greater pressure on the anterior than on the posterior side of a solid moving uniformly relatively to a fluid. A symmetrical solid, as for example a globe, moving uniformly through a frictionless fluid, experiences augmentation of pressure in front and behind equally; and diminished pressure over an intervening zone. Observation (as for instance in Mr. J. R. Napier's experiments on his " pressure log," for measuring the speed of vessels, and experiments by Joule and myself*, on the pressure at different points of a solid globe exposed to wind) shows that, instead of being increased, the pressure is sometimes actually diminished on the posterior side of a solid moving through a real fluid such as air or water. Wind blowing across ridges and hollows of a fixed solid (such as the furrows of a field) must, because of the viscosity of the air, press with greater force on the slopes facing it than on the sheltered slopes. Hence if a regular series of waves at sea consisted of a solid body moving with the actual velocity of the waves, the wind would do work upon it, or it would do work upon the air, according as the velocity of the wind were greater or less than the velocity of the waves. This case does not afford an exact parallel to the influence of wind on waves, because the surface particles of water do not move forward with the velocity of the waves as those of the furrowed solid do. Still it may be expected that when the velocity of the wind exceeds the velocity of propagation of the waves, there will be a greater pressure on the posterior slopes than on the anterior slopes of the waves; and *vice versâ*, that when the velocity of the waves exceeds the velocity of the wind, or is in the direction opposite to that of the wind, there will be a greater pressure on the anterior than on the posterior slopes of the waves. In the first case the tendency will be to augment the wave, in the second case to diminish it. The question whether a series of waves of a certain height gradually augment with a certain force of wind or gradually subside through the wind not being strong enough to sustain them, cannot be decided offhand. Towards answering it Stokes's investigation of the work against viscosity of water required to maintain a wave†, gives a most important and suggestive instal-

* " Thermal Effects of Fluids in Motion," Royal Society Transactions, 1860; and Phil. Mag. 1860, vol. xx. p. 552.

† Transactions of the Cambridge Philosophical Society, 1851 (" Effect of Internal Friction of Fluids on the Motion of Pendulums," Section V.).

ment. But no theoretical solution, and very little of experimental investigation, can be referred to with respect to the eddyings of the air blowing across the tops of the waves, to which, by its giving rise to greater pressure on the posterior than on the anterior slopes, the influence of the wind in sustaining and maintaining waves is chiefly if not altogether due.

My attention having been called three days ago, by Mr. Froude, to Scott Russell's Report on Waves (British Association, York, 1844), I find in it a remarkable illustration or indication of the leading idea of the theory of the influence of wind on waves, that the velocity of the wind must exceed that of the waves, in the following statement :—" Let him [an observer studying the surface of a sea or large lake, during the successive stages of an increasing wind, from a calm to a storm] begin his observations " in a perfect calm, when the surface of the water is smooth and " reflects like a mirror the images of surrounding objects. This " appearance will not be affected by even a slight motion of the " air, and a velocity of less than half a mile an hour ($8\frac{1}{2}$ in. per " sec.) does not sensibly disturb the smoothness of the reflecting " surface. A gentle zephyr flitting along the surface from point " to point, may be observed to destroy the perfection of the mir- " ror for a moment, and on departing, the surface remains po- " lished as before ; if the air have a velocity of about a mile an " hour, the surface of the water becomes less capable of distinct " reflexion, and on observing it in such a condition, it is to be " noticed that the diminution of this reflecting power is owing " to the presence of those minute corrugations of the superficial " film which form waves of the *third order*. These corrugations " produce on the surface of the water an effect very similar to " the effect of those panes of glass which we see corrugated for " the purpose of destroying their transparency, and these corru- " gations at once prevent the eye from distinguishing forms at a " considerable depth, and diminish the perfection of forms re- " flected in the water. To fly-fishers this appearance is well " known as diminishing the facility with which the fish see their " captors. This first stage of disturbance has this distinguishing " circumstance, that the phenomena on the surface cease almost " simultaneously with the intermission of the disturbing cause, " so that a spot which is sheltered from the direct action of the " wind remains smooth, the waves of the third order being inca- " pable of travelling spontaneously to any considerable distance, " except when under the continued action of the original disturb- " ing force. This condition is the indication of present force, " not of that which is past. While it remains it gives that deep " blackness to the water which the sailor is accustomed to regard " as an index of the presence of wind, and often as the forerunner " of more.

" The second condition of wave motion is to be observed when
" the velocity of the wind acting on the smooth water has in-
" creased to two miles an hour. Small waves then begin to rise
" uniformly over the whole surface of the water; these are waves
" of the second order, and cover the water with considerable re-
" gularity. Capillary waves disappear from the ridges of these
" waves, but are to be found sheltered in the hollows between
" them, and on the anterior slopes of these waves. The regu-
" larity of the distribution of these secondary waves over the
" surface is remarkable; they begin with about an inch of am-
" plitude, and a couple of inches long; they enlarge as the ve-
" locity or duration of the wave increases; by and by contermi-
" nal waves unite; the ridges increase, and if the wind increase
" the waves become cusped, and are regular waves of the *second*
" *order*.   They continue enlarging their dimensions; and the
" depth to which they produce the agitation increasing simulta-
" neously with their magnitude, the surface becomes extensively
" covered with waves of nearly uniform magnitude."

The " Capillary waves " or " waves of the third order " re-
ferred to by Russell are what I, in ignorance of his observations
on this branch of his subject, had called " ripples." The velocity
of $8\frac{1}{2}$ inches ($21\frac{1}{2}$ centimetres) per second is precisely the velo-
city he had chosen (as indicated by his observations) for the
velocity of propagation of the straight-ridged waves streaming
obliquely from the two sides of the path of a small body moving
at speeds of from 12 to 36 inches per second; and it agrees re-
markably with my theoretical and experimental determination of
the absolute minimum wave-velocity (23 centimetres per second;
see Part V.). Russell has not explicitly pointed out that his
critical velocity of $8\frac{1}{2}$ inches per second was an absolute mini-
mum velocity of propagation. But the idea of a minimum velo-
city of waves can scarcely have been far from his mind when he
fixed upon $8\frac{1}{2}$ inches per second as the minimum of wind that
can sustain ripples. In an article to appear in ' Nature ' on the
26th of this month, I have given extracts from Russell's Report
(including part of a quotation which he gives from Poncelet and
Lesbros in the memoirs of the French Institute) for 1829, show-
ing how far my observations on ripples had been anticipated.
I need say no more here than that these anticipations do not
include any indication of the dynamical theory which I have
given, and that the subject was new to me when Parts III.,
IV., and V. of the present communication were written.

PART V. *Waves under motive power of Gravity and Cohesion*
jointly, without wind.

Leaving the question of wind, consider (13), and introduce

notation of (16), (17) in it. It becomes

$$w^2 = \frac{g'}{n} + \mathrm{T}'n. \qquad \ldots \qquad (19)$$

This has a minimum value,

when

$$\left. \begin{aligned} w^2 &= 2\sqrt{g'\mathrm{T}'}, \\ n &= \sqrt{\frac{g'}{\mathrm{T}'}}. \end{aligned} \right\} \qquad \ldots \qquad (20)$$

In applying these formulæ to the case of air and water, we may neglect the difference between g and g', as the value of σ is about $\frac{1}{820}$; and between T and T', although it is to be remarked that it is T' rather than T that is ordinarily calculated from experiments on capillary attraction. From experiments of Gay-Lussac's it appears that the value of T' is about ·074 of a gramme weight per centimetre; that is to say, in terms of the kinetic unit of force founded on the gramme as unit of mass,

$$\mathrm{T}' = g \times \text{·}073.$$

To make the density of water unity (as that of the lower liquid has been assumed), we must take one centimetre as unit of length. Lastly, with one second as unit of time, we have

$$g = 982;$$

and (18) gives

$$w = \sqrt{982\left(\frac{1}{n} + \text{·}074 \times n\right)}$$

for the wave-velocity in centimetres per second, corresponding to wave-length $\frac{2\pi}{n}$. When $\frac{1}{n} = \sqrt{\text{·}073} = \text{·}27$ (that is, when the wave-length is 1·7 centimetre), the velocity has a minimum value of 23 centimetres per second.

The part of the preceding theory which relates to the effect of cohesion on waves of liquids occurred to me in consequence of having recently observed a set of very short waves advancing steadily, directly in front of a body moving slowly through water, and another set of waves considerably longer following steadily in its wake. The two sets of waves advanced each at the same rate as the moving body; and thus I perceived that there were two different wave-lengths which gave the same velocity of propagation. When the speed of the body's motion through the water was increased, the waves preceding it became shorter, and those in its wake became longer. Close before the cut-water of

a vessel moving at a speed of not more than two or three knots *
through very smooth water, the surface of the water is marked
with an exquisitely fine and regular fringe of ripples, in which
several scores of ridges and hollows may be distinguished (and
probably counted, with a little practice) in a space extending 20
or 30 centimetres in advance of the solid. Right astern of either
a steamer or sailing vessel moving at any speed above four or
five knots, waves may generally be seen following the vessel at
exactly its own speed, and appearing of such lengths as to verify
as nearly as can be judged the ordinary formula

$$l = \frac{2\pi w^2}{g}$$

for the length of waves advancing with velocity w, in deep
water. In the well-known theory of such waves, gravity is as-
sumed as the sole origin of the motive forces. When cohesion
was thought of at all (as, for instance, by Mr. Froude in his im-
portant nautical experiments on models towed through water, or
set to oscillate to test qualities with respect to the rolling of ships
at sea), it was justly judged to be not sensibly influential in
waves exceeding 5 or 10 centimetres in length. Now it becomes
apparent that for waves of any length less than 5 or 10 centi-
metres cohesion contributes sensibly to the motive system, and
that, when the length is a small fraction of a centimetre, cohesion
is much more influential than gravity as "motive" for the vi-
brations.

The following extract from part of a letter to Mr. Froude,
forming part of a communication to 'Nature' (to appear on the
26th of this month), describes observations for an experimental
determination of the minimum velocity of waves in sea-water :—

 "About three weeks later, being becalmed in the Sound of
" Mull, I had an excellent opportunity, with the assistance of
" Professor Helmholtz, and my brother from Belfast, of deter-
" mining by observation the minimum wave-velocity with some
" approach to accuracy. The fishing-line was hung at a distance
" of two or three feet from the vessel's side, so as to cut the
" water at a point not sensibly disturbed by the motion of the
" vessel. The speed was determined by throwing into the sea
" pieces of paper previously wetted, and observing their times of
" transit across parallel planes, at a distance of 912 centimetres
" asunder, fixed relatively to the vessel by marks on the deck
" and gunwale. By watching carefully the pattern of ripples
" and waves which connected the ripples in front with the waves
" in rear, I had seen that it included a set of parallel waves

 * The speed "one knot" is a velocity of one nautical mile per hour, or
51·5 centimetres per second.

" slanting off obliquely on each side, and presenting appearances
" which proved them to be waves of the critical length and cor-
" responding minimum speed of propagation. Hence the com-
" ponent velocity of the fishing-line perpendicular to the fronts
" of these waves was the true minimum velocity. To measure
" it, therefore, all that was necessary was to measure the angle
" between the two sets of parallel lines of ridges and hollows
" sloping away on the two sides of the wake, and at the same
" time to measure the velocity with which the fishing-line was
" dragged through the water. The angle was measured by hold-
" ing a jointed two-foot rule, with its two branches, as nearly as
" could be judged by the eye, parallel to the set of lines of wave
" ridges. The angle to which the ruler had to be opened in this
" adjustment was the angle sought. By laying it down on paper,
" drawing two straight lines by its two edges, and completing a
" simple geometrical construction with a length properly intro-
" duced to represent the measured velocity of the moving solid,
" the required minimum wave-velocity was readily obtained.
" Six observations of this kind were made, of which two were
" rejected as not satisfactory. The following are the results of
" the other four :—

Velocity of moving solid.			Deduced minimum wave-velocity.		
51 centimetres per second.			23·0 centimetres per second.		
38	,,	,,	23·8	,,	,,
26	,,	,,	23·2	,,	,,
24	,,	,,	22·9	,,	,,
		Mean . .	23·22		

" The extreme closeness of this result to the theoretical esti-
" mate (23 centimetres per second) was, of course, merely a co-
" incidence ; but it proved that the cohesive force of sea-water
" at the temperature (not noted) of the observation cannot be
" very different from that which I had estimated from Gay-
" Lussac's observations for pure water."

XLVII. *Preliminary Catalogue of the Bright Lines in the Spec-
trum of the Chromosphere. By* C. A. YOUNG, *Ph.D., Professor
of Astronomy in Dartmouth College*[*].

THE following list contains the bright lines which have been
observed by the writer in the spectrum of the chromo-
sphere within the past four weeks. It includes, however, only

those which have been seen twice at least; a number observed on one occasion (September 7) still await verification.

The spectroscope employed is the same described in the Journal of the Franklin Institute for November 1870; but certain important modifications have since been effected in the instrument. The telescope and collimator have each a focal length of nearly 10 inches, and an aperture of $\frac{7}{8}$ of an inch. The prism-train consists of five prisms (with refracting angles of $55°$) and two half-prisms. The light is sent twice through the whole series by means of a prism of total reflection at the end of the train, so that the dispersive power is that of twelve prisms. The instrument distinctly divides the strong iron line at 1961 of Kirchhoff's scale, and separates B (not b) into its three components. Of course it easily shows every thing that appears on the spectrum-maps of Kirchhoff and Ångström. The adjustment for "the position of minimum deviation" is automatic; *i. e.* the different portions of the spectrum are brought to the centre of the field of view by a movement which at the same time also adjusts the prisms.

The telescope to which the spectroscope is attached is the new equatorial recently mounted in the observatory of the College by Alvan Clark and Sons. It is a very perfect specimen of the admirable optical workmanship of this celebrated firm, and has an aperture of $9\frac{4}{10}$ inches, with a focal length of 12 feet.

In the Table the first column contains simply the reference number. An asterisk denotes that the line affected by it has no well-marked corresponding dark line in the ordinary solar spectrum.

The second column gives the position of the line upon the scale of Kirchhoff's map, determined by direct comparison with the map at the time of observation. In some cases an interrogation-mark is appended, which signifies not that the *existence* of the line is doubtful, but only that its precise place could not be determined, either because it fell in a shading of fine lines, or because it could not be decided in the case of some close double lines which of the two components was the bright one, or, finally, because there were no well-marked dark lines near enough to furnish the basis of reference for a perfectly accurate determination.

The third column gives the position of the line upon Ångström's normal atlas of the solar spectrum. In this column an occasional interrogation-mark denotes that there is some doubt as to the precise point of Ångström's scale corresponding to Kirchhoff's. There is considerable difference between the two maps, owing to the omission of many faint lines by Ångström, and the want of the fine gradations of shading observed by

Preliminary Catalogue of Chromospheric Lines.

Reference number.	Kirchhoff.	Ångström.	Relative frequency.	Relative brightness.	Chemical element.	Previous observer.
*1.	534·5	7060·0?	60	3	......	
2.	654·5	6677·0?	8	4	......	L.
3.	C	6561·8	100	100	H	L. J.
4.	719·0	6495·7	2	2	Ba	
5.	734·0	6454·5	2	3	......	
6.	743·?	6431·0	2	2		
7.	768·?	6370·0	2	2		
8.	816·8	6260·3	1	1	Ti	
9.	820·0	6253·2	1	2	Fe	
10.	874·2	6140·5	6	8	Ba	L.
11.	D_1	5894·8	10	10	Na	L.
12.	D_2	5889·0	10	10	Na	L.
*13.	1017·0	5871·0	100	75	......	L. J.
14.	1274·3	5534·0	6	8	Ba	R. L.
15.	1281·5	5526·0	1	1	Fe	
16.	1343·5	5454·5	1	2	Fe	
17.	1351·3	5445·9	1	2	Fe, Ti	
18.	1363·1	5433·0	1	1	Fe	
*19.	1366·0	5430·0	2	3		
20.	1372·0	5424·5	3	4	Ba	L.
21.	1378·5?	5418·0?	1	2	Ti?	
*22.	1382·5	5412·0	1	1		
23.	1391·2	5403·0	2	2	Fe, Ti	
24.	1397·8	5396·2	1	2	Fe	
25.	1421·5	5370·4	1	2	Fe	R.
26.	1431·3	5360·6	2	2	......	R.
27.	1454·7	5332·0	2	2	Ti	
28.	1462·9	5327·7	1	3	Fe	
29.	1463·4	5327·2	1	3	Fe	
30.	1465·0?	5321·0	2	2		
31.	Corona line 1474·1	5315·9	75	15	Fe?	L.
32.	1505·5	5283·0	5	4		
33.	1515·5	5275·0	7	5	......	L. R.
34.	E_1	5269·5	1	3	Fe, Ca	
35.	F_2	5268·5	1	2	Fe	
36.	1528·0	5265·5	3	2	Fe, Co	L.
37.	1561·0	5239·0	1	1	Fe	
38.	1564·1	5236·2	1	1	......	
39.	1567·7	5233·5	2	2	Mn	R.
40.	1569·7	5232·0	1	2	Fe	
41.	1577·3	5226·0	1	2	Fe	
42.	1580·5?	5224·5	1	1	Ti?	
43.	1601·5	5207·3	3	3	Cr, Fe?	
44.	1604·4	5205·3	3	3	Cr	
45.	1606·5	5203·7	3	3	Cr, Fe?	
46.	1609·3	5201·6	1	2	Fe	
47.	1611·5	5199·5	1	1		
48.	1615·6	5197·0	3	2	......	L. R.
49.	b_1	5183·0	15	15	Mg	L.
50.	b_2	5172·0	15	15	Mg	L.
51.	b_3	5168·5	12	10	Ni	L.
52.	b_4	5166·5	10	10	Mg	L.

Reference number.	Kirchhoff.	Ångström.	Relative frequency.	Relative brightness.	Chemical element.	Previous observer.
53.	1673·9	5153·2	1	1	Na	
54.	1678·0	5150·1	1	2	Fe	
55.	1778·5	5077·8	1	1	Fe	
56.	1866·8	5017·5	2	3	......	R.
57.	1870·3	5015·0?	2	2	......	R.
58.	1989·5	4933·4	8	5	Ba	L.
59.	2001·5	4923·2	5	3	Fe	R. L
60.	2003·2	4921·3	1	1		
61.	2007·1	4918·1	3	3	......	L.
62.	2031·0	4899·3	6	4	Ba	L.
63.	2051·5	4882·5	2	2	......	L.
64.	F	4860·6	100	75	H	J. L.
65.	2358·5	4629·0	1	1	Ti	
66.	2419·3	4583·5	1	1		
67.	2435·5	4571·4	1	1	Li	
68.	2444·0	4564·6	1	1		
69.	2446·6	4563·1	1	2	Ti	
70.	2457·8	4555·0	1	1	Ti	
71.	2461·2	4553·3	3	3	Ba	
72.	2467·7	4548·7	1	3	Ti	
73.	2486·8	4535·2	1	1	Ti, Ca?	
74.	2489·5	4533·2	1	1	Fe	
75.	2490·6	4531·7	1	1	Ti	
76.	2502·5	4524·2	2	2	Ba	
77.	2505·8	4522·1	1	2	Ti	
78.	2537·3	4500·4	1	3	Ti	
79.	2553·0?	4491·0?	1	1	Mn?	
80.	2555·0?	4489·5?	1	1	Mn?	
81.	2566·5	4480·4	1	2	Mg	L.
82.	2581·5?	4471·4	75	8	{A band rather than a line.	
83.	2585·5	4468·6	1	1	Ti	
84.	2625·0	4443·0	1	1	Ti	
85.	2670·0	4414·6	1	1	Fe, Mn	
86.	2686·7	4404·3	1	2	Fe	
87.	2705·0	4393·5	3	2	Ti	
88.	2719·0?	4384·8	1	1	Ca?	
89.	2721·2	4382·7	1	2	Fe	
90.	2734·0?	4372·0	1	1		
91.	2737·0?	4369·3?	1	1	Cr ?	
92.	2775·8	4352·0	1	1	Fe, Cr	
93.	2796·0	4340·0	100	50	H	L. J
94.	G	4307·0	1	2	Fe, Ti, Ca	
95.	2770·0	4300·0	1	1	Ti	
96.		4297·5	1	1	Ti, Ca	
97.		4289·0	1	2	Cr	
98.		4274·5	1	2	Cr	
99.		4260·0	1	1	Fe	
100.		4245·2	1	1	Fe	
101.		4226·5	1	1	Ca	
102.		4215·5	1	2	Fe, Ca	
103.	h	4101·2	100	20	H	R. L

Kirchhoff, which renders the coordination of the two scales some-
times difficult, and makes the atlas of Kirchhoff far superior to
the other for use in the observatory.

The numbers in the fourth column are intended to denote the
percentage of frequency with which the corresponding lines are
visible in my instrument. They are to be regarded as only
roughly approximative; it would, of course, require a much
longer period of observation to furnish results of this kind
worthy of much confidence.

In the fifth column the numbers denote the relative brilliance
of the lines on a scale where 100 is the brightest and 1 the faint-
est. These numbers also, like those in the preceding column,
are entitled to very little weight.

The sixth column contains the symbols of the chemical sub-
stances to which, according to the maps above referred to, the
lines owe their origin.

There are no disagreements between the two authorities; in
the majority of cases, however, Ångström alone indicates the
element; and there are several instances where the lines of more
than one substance coincide with each other and with a line of
the solar spectrum so closely as to make it impossible to decide
between them.

In the seventh and last column the letters J., L., and R. de-
note that, to my knowledge, the line indicated has been observed
and its place published by Janssen, Lockyer, or Rayet. It is
altogether probable that a large portion of the other lines con-
tained in the catalogue have before this been seen and located by
one or the other of these keen and active observers; but if so, I
have as yet seen no account of such determinations.

I would call especial attention to the lines numbered 1 and
82 in the catalogue; they are very persistently present, though
faint, and can be distinctly seen in the spectroscope to belong to
the chromosphere as such, not being due, like most of the other
lines, to the exceptional elevation of matter to heights where it
does not properly belong. It would seem very probable that
both these lines are due to the same substance which causes the
D^3 line.

I do not not know that the presence of titanium vapour in
the prominences and chromosphere has before been ascertained.
It comes out very clearly from the catalogue, as no less than 20
of the whole 103 lines are due to this metal.

Hanover, N. H., September 13, 1871.

XLVIII. *Notices respecting New Books.*

Explanatory Mensuration for the use of Schools. Containing numerous examples, and (by the kind permission of the Oxford Delegates) embodying nearly all the questions set in their local Examination Papers. By the Rev. ALFRED HILEY, *M.A.* London : Longmans and Co. 1871. Pp. 158.

IT is much to be regretted that the Oxford Delegates gave Mr. Hiley permission to use their questions. By doing so they have given a sort of informal authority to a very poor book. Several parts of the subject are included that are very ill adapted for boys whose knowledge of mathematics is limited to arithmetic— such as the Mensuration of Segments of Spheres, Frustums of Wedges, &c. But this is by no means the worst point of the book. Mr. Hiley's statements and explanations are frequently awkwardly expressed and inexact. Thus he defines a right-angled triangle as one " that *contains* a right angle " (p. 3). He classifies lines in the following queer fashion :—" Lines may be either straight, curved, or parallel " (p. 1). He lays it down that " The circumference of any circle is divided into 360 *parts* called degrees " (p. 6), instead of " 360 *equal* parts." If an arc A C B subtends at the centre of a circle an angle A E B, he tells us that " the arc A C B bears the same ratio to the circumference of the circle that *the angle* A E B does to 360° " (p. 70), instead of " the number of degrees in the angle A E B ;" and so on in many other cases.

Occasionally his inexactness wanders into inaccuracy, as in the following case (p. 80) :—" To find the circumference or perimeter of the ellipse. Multiply half the sum of the two diameters [he means the two *principal axes*] by $\frac{22}{7}$." If Mr. Hiley will apply his rule to the case in which the minor axis is indefinitely small, when the perimeter will equal twice the major axis, he will easily deduce the curious arithmetical theorem that

$$14 = 11,$$

or, in accordance with the rule provided " when greater accuracy is desirable " (p. 81), that

$$4 = 3{\cdot}1416.$$

Mr. Hiley's account of the prismoid is given in such a form that no one who comes fresh to the subject could apply it to the determination of the volume of a portion of a railway-cutting ; yet three of his examples contemplate this application. He is particularly unfortunate in these examples. One of them (which is due to the Oxford Delegates) is correctly set, and the answer is correct. The two other examples were apparently drawn up by Mr. Hiley himself. But if he made a model of the solids referred to in his questions, he would find that one or both of the slant faces of the cuttings would be not planes but curved surfaces of some kind or other, and, as these curved surfaces are not defined, the questions do not admit of answers.

We have by no means exhausted the list of Mr. Hiley's inaccuracies. We have, however, said enough to warn teachers from adopting it in middle-class schools. We fear that it may be adopted; for it is cheap, it will probably be puffed by ignorant writers, and it comes out with an *apparent* sanction from the Oxford Delegates. But if it is adopted, many an intelligent boy will have all the training he ever gets in an exact science of a far less valuable kind than it might have been had his teacher put into his hands a well-written treatise on " Explanatory Mensuration."

XLIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 228.]

June 15, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communication was read :—
    " On the Measurement of the Chemical Intensity of Total Daylight made at Catania during the Total Eclipse of December 22, 1870." By Henry E. Roscoe, F.R.S., and T. E. Thorpe, F.R.S.E.

The following communication contains the results of a series of measurements of photochemical action made at Catania in Sicily, on Dec. 22nd, 1870, during the total solar eclipse of that date, with the primary object of determining experimentally the relation existing between this action and the changes of area in the exposed portion of the sun's disk. The attempt to establish this relation has already been made by one of us from the results of observations carried out by Captain John Herschel, R.E., F.R.S., at Jamkandi, in India, during the total eclipse of Aug. 18, 1868. Unfortunately the state of the weather at Jamkandi at the time of the eclipse was very unfavourable, and the results were therefore not of so definite a character as could be desired, and it appeared important to verify them by further observation. The method of measurement adopted is that described by one of us in the Bakerian Lecture for 1865 ; the observations were made in the Garden of the Benedictine Monastery of San Nicola, at Catania, the position of which, according to the determination of Mr. Schott of the United States' Coast Survey, is lat. 37° 30′ 12″ N., long. 1ʰ 0′ 18″ E. In order to obtain data for determining the variation in chemical intensity caused by the alteration in the sun's altitude during the eclipse, observations were made on the three previous days, during which the sky was perfectly cloudless.

In the following Table the observations taken at about the same hours are grouped together :—

Table I.

Mean Alti- tude.	No. of Ob- servations.	Chemical Intensity.		
		Diffused.	Direct.	Total.
1 30 28	1	0·009	0·000	0·009
9 28 10	7	0·044	0·008	0·052
13 9 57	7	0·050	0·014	0·064
19 57 49	12	0·072	0·028	0·100
24 46 12	7	0·095	0·049	0·144
28 24 10	14	0·108	0·047	0·155

The above numbers confirm the conclusion formerly arrived at, viz. that the relation between total chemical intensity and sun's altitude is represented by a straight line, or by the equation

$$CI_a = CI_0 + \text{const.} \times a,$$

where CI_a signifies the chemical intensity at any altitude a in circular measure, CI_0 the chemical intensity at $0°$, and const. a a number de- rived from the observations.

The observations on the day of the eclipse (the 22nd) were com- menced about nine o'clock A.M., and up to the time of first contact were made regularly at intervals of about an hour. The sky up to this point was cloudless, and the measurements almost absolutely coincided with the mean numbers of the preceding day's observa- tions. As the eclipse progressed, and the temperature of the air fell, clouds were rapidly formed, and from $1^h 40'$ up to the time of totality it was impossible to make any observations, as the sun was never unclouded for more than a few seconds at a time. As the illu- minated portion of the solar disk gradually increased after totality, the clouds rapidly disappeared, the amount falling from 9 (overcast $= 10$) to 3 in about fifteen minutes. The observations were then regularly continued to within a few minutes of last contact.

Although the disk and by far the largest portion of the heavens were completely obscured by clouds during totality, rendering any determination of the photochemical action perfectly valueless for our special object, it was yet thought worth while to attempt to estimate the chemical intensity of the feebly diffused light at this time, which certainly is capable of producing photographic action.

Immediately after the supposed commencement of totality the slit was opened, and the sensitive paper exposed for ninety-five seconds. Not the slightest action, however, could be detected on the paper ; and we therefore believe that we are correct in estimating the inten- sity of the chemically active light present at certainly not more than 0·003 of the unit which we adopt, and probably much less.

The Table containing the experimental numbers and the graphic representation of them are given in the memoir. By a graphical

method the relative areas of the sun uneclipsed at the times of observation were obtained; and these are seen in column 3 of Table II., the area of the unobscured sun being taken as unity.

Column 2 gives the results of the photochemical observations made during the eclipse, obtained from the graphical mean, and corrected for variation in the sun's altitude, the total chemical action immediately before first contact being taken as unity. Column 1 gives the apparent solar times of observation.

Table II.

1.	2.	3.
$\overset{h}{12} \overset{,}{44}$	0·915	0·961
12 54	0·876	0·880
1 16	0·686	0·637
1 24	0·555	0·534
2 2	0·000	0·000
2 9	0·165	0·127
2 25	0·307	0·338
2 34	0·464	0·498
2 44	0·601	0·602
2 54	0·725	0·736
3 4	0·876	0·861

From these observations we conclude *that the diminution in the total chemical intensity of the sun's light during an eclipse is directly proportional to the magnitude of the obscuration.*

The question of the variation of (1) the direct and (2) the diffused radiation is next discussed. On comparing the curve representing the chemical intensity of diffused light with the curve of solar obscuration, it is found that the rate of diminution in chemical action exerted by the diffused light is up to a certain point greater than corresponds to the portion of eclipsed sun, whilst from this point up to totality the rate of diminution becomes less than corresponds to the progress of the eclipse. The same rapid diminution in the chemical action of the diffused daylight during the early periods of the eclipse was also observed at Jamkandi; it is doubtless due to the dark body of the moon cutting off the light from the brightly illuminated portion of sky lying round the solar disk.

The results of the observations at Catania are then compared with those made at Moita, near Lisbon, and communicated to the Society in 1870. This comparison shows a striking coincidence between the two sets of observations. In each case it is seen that the relation between solar altitude and total chemical intensity is represented by a straight line, although the Catania observations slightly exceed, by a constant difference, those made at Moita, in conformity with the slight difference in latitude, and with the fact that the former determinations were made at a greater elevation above the sea-level.

The Catania observations further confirm the fact which we formerly announced, that for altitudes below 50° the amount of che-

mical action effected in the plane of the horizon by diffused daylight
is greater than that exerted by direct radiation, and also that at alti-
tudes below $10°$ direct sunlight is almost completely robbed of its
chemically active rays.

GEOLOGICAL SOCIETY.

[Continued from p. 318.]

April 26, 1871.—Prof. Morris, Vice-President, in the Chair.

The following communications were read :—

1. " On a new species of Coral from the Red Crag of Waldring-
field." By Prof. P. Martin Duncan, M.B., F.R.S., F G.S.

Prof. Duncan described, under the name of *Solenastræa Prestwichi*,
a small compound Coral obtained by Mr. A. Bell from Waldringfield,
and stated that it was particularly interesting as belonging to a
reef-forming type of corals which has persisted at least from the
Eocene period to the present day. The single specimen consisted of
several small crowded corallites, having calices from $\frac{1}{20}$ to $\frac{3}{20}$ inch
in diameter, united by a cellular epithecal cœnenchyma. It was
much rolled and worn before its deposition in the Red Crag, and
hence the author regarded it as a derivative fossil in that formation ;
and he stated that it probably belonged to the rich reef-building
coral-fauna which succeeded that of the Nummulitic period.

2. " Notes on the Minerals of Strontian, Argyllshire." By Robert
H. Scott, Esq., M.A., F.R.S., F.G.S.

The paper stated that the existing lists of minerals to be found
at Strontian were incorrect. The discovery of apophyllite, talc,
and zircon seemed to be hardly sufficiently confirmed. On the
other hand, Mr. Scott named several species which he had himself
observed *in situ*, and which are not noticed in any of the books, viz. :—
two felspars, orthoclase, and an anorthic felspar in the granite ;
two varieties of pyroxenic minerals in the granites and syenites,
neither of which have as yet been analyzed ; natrolite in the trap-
dykes, muscovite or margarodite in very large plates, lepidome-
lane and schorl.

Specimens of these minerals and of the others found at the mines
were exhibited ; but it was stated that, owing to the fact that the
old workings at the mines in Glen Strontian had been allowed to
fall in, it was now no longer possible to ascertain much about the
association of the species.

The one is galena, containing very little silver. The gangue is
remarkable for the absence of fluor and the comparative rarity of
blende and heavy spar. Harmotome is found principally at a
mine called Bell's Grove, both in the opaque variety and in the
clear one called morvenite. Brewsterite occurs at the mine called
Middle Shap ; and at the mine Whitesmith strontianite is found
with brewsterite, but without harmotome. Calcite is also very
common.

Within the last few years a new mine has been opened, called

Corrantee, which is in the gneiss, whereas the other mines lie on the junction of the granite and gneiss. At this mine several fine specimens of calcite have occurred, many of them coated with twin crystals of harmotome, similar to those from Andreasberg, whereas the crystals found at the old mine are not so clearly macled.

Associated with these were found a number of small hexagonal prisms, perfectly clear, and exhibiting a very obtuse dihedral termination. They gave the blowpipe reaction of harmotome, and, on analysis by Dr. J. E. Reynolds, proved to be that mineral.

Descloiseaux has already described a quadrifacial termination to harmotome, with an angle of 178° 20'.

Mr. Scott submitted that possibly the crystals which he exhibited might bear faces which had a close relation to those described by Descloiseaux.

He concluded by stating that Strontian promised as rich a harvest to the mineralogist as any locality in these islands.

3. " On the probable origin of Deposits of ' Loess' in North China and Eastern Asia." By T. W. Kingsmill, Esq., of Shanghai.

The author stated that the Baron von Richthofen had lately applied the term " Loess " to a light clay deposit covering immense tracts in the north of China. The author regarded this formation as in great measure corresponding to the Kunkur of India, and thought that it probably extended far into the elevated plains of Central Asia. Richthofen considered that this deposit had been produced by subaërial action upon a surface of dry land; the author argued that it is of marine origin, having been deposited when the region which it covers was depressed at least 6000 feet, a depression the occurrence of which since the commencement of the Tertiary period he considered to be proved by the mode of deposition of the Upper Nanking Sandstones and Conglomerates, the bold escarpments of the hills on either side of the Yangtsze, and other peculiarities of the country.

May 10, 1871.—Prof. Morris, Vice-President, in the Chair.

The following communications were read:—

1. " On the Ancient Rocks of the St. David's Promontory, South Wales, and their Fossil contents." By Prof. R. Harkness, F.R.S., F.G.S., and Henry Hicks, Esq.

In the Promontory of St. David's the rocks upon which the conglomerates and purple and greenish Sandstone, forming the series usually called the " Longmynd " and " Harlech Groups," repose, are highly quartziferous, and in many spots so nearly resemble syenite that it is at first difficult to make out their true nature. The apparent crystals, however, are for the most part angular fragments of quartz, not possessing the true crystalline form of the mineral. The matrix does not exhibit a crystalline arrangement, and contains a very large proportion of silica, much exceeding that which is obtained from rocks of a syenitic nature. These quartziferous rocks form an E.N.E. and W.S.W. course. The arrangement of these rocks, which seem to be quartziferous breccias, is somewhat indistinct.

In the immediate neighbourhood of St. David's they have associated with them irregular bands of hard, greenish, ashy-looking shales, much altered in character, but often presenting distinct traces of foliation. In a ridge running from the S.E. of Ramsey Sound in a north-easterly direction the greenish shales are more compact, and resemble earthy greenstones.

The quartziferous breccias and their associated shales form two anticlinal axes, contiguous to each other, and have on their S.S.E. and N.N.W. sides purple and green rocks.

The order of the rocks from the quartziferous breccias upwards, when not disturbed by faults, is as follows :—

Lower Cambrian.

1. Greenish hornstones on the S.E., and earthy Greenstones on the N.W., forming the outermost portions of the so-called Syenitic and Greenstone ridges.

	feet.
2. Conglomerates, composed chiefly of well-rounded masses of quartz imbedded in a purple matrix.	60
3. Greenish flaggy sandstones	460
4. Red flaggy or shaly beds, affording the earliest traces of organic remains in the St. David's Promontory, namely *Lingulella ferruginea* and *Leperditia cambrensis*	50
5. Purple (sometimes greenish) sandstones	1000
6. Yellowish-grey sandstones, shales, and flags, containing the genera *Plutonia, Conocoryphe, Microdiscus, Agnostus, Theca,* and *Protospongia*	150
7. Grey, purple, and red flaggy sandstones, containing, with some of the above-mentioned genera, the genus *Paradoxides*	1500
8. Grey flaggy beds	150
9. The true beds of the "Menevian Group," richly fossiliferous, and the probable equivalents of the lowest portions of the primordial zone of M. Barrande	550

The discovery of a fauna (specially rich in trilobites) among these rocks of the St. David's Promontory affords very important information concerning the earlier forms of life of the British Isles. Until the discovery of this fauna, these rocks and their equivalents in North Wales were looked upon as all but barren of fossils. We have now, scattered through about 3000 feet of purple and green strata, a well-marked series of fossils, such as have nowhere else been obtained in the British Isles. In the Longmynds of Shropshire the only evidence of the existence of life during the period of their deposition is in the form of worm-burrows, and in the somewhat indistinct impressions which Mr. Salter regards as trilobitic, and to which he has given the name of *Palæopyge Ramsayi.* If we assume the purple and green shales and sandstones, with their associated quartz rocks of Bray Head and the drab shales of Carrick M'Reily, county Wicklow, to represent the old rocks of St. David's, they afford only very meagre evidence of the occurrence of life during the period of their deposition, in the form of worm-burrows and tracks, and in the very indeterminate fossils which have been referred to the genus *Oldhamia.*

One very prominent feature about the palæontology of the ancient rocks of St. David's is the occurrence of four distinct species of the genus *Paradoxides* ; and this is in strong contrast with the entire

absence of the genus *Olenus*. On a comparison of the palæontology of the St. David's rocks with those of the continent of Europe and of America which seem to occupy nearly the same horizon, we have like features to a very great extent presenting themselves.

With reference to the distribution in time of some of the earlier genera of trilobites, it would appear that the genus *Olenus* is represented in Britain and Europe by twenty-two species, confined to the Lingula-flags and Tremadoc rocks, and not occurring so low as the Menevian group. The absence of this genus from the Menevian group, and its occurrence throughout the whole of the Lingula-flags, and in the Tremadoc rocks, along with the fact that, so far as present observations go, no species of *Paradoxides* ranges higher than the Menevian group, have afforded good palæontological grounds for placing the line of demarcation between Upper and Lower Cambrian at this spot, and for including the Menevian group in the Lower Cambrian, to the bulk of which it is intimately united palæontologically.

2. " On the Age of the Nubian Sandstone." By Ralph Tate, Esq., F.G.S.

The author remarked that the sandstone strata underlying the Cretaceous limestones, and resting upon the granitic and schistose rocks of Sinai, had been identified with the " Nubian Sandstone " described by Russegger as occurring in Egypt, Nubia, and Arabia Petræa. In the absence of palæontological evidence, this sandstone has been referred to the Mesozoic group, having been regarded by Russegger as Lower Cretaceous, and by Mr. Bauerman and Figari-Bey as Triassic, the latter considering an intercalated limestone-bed to be the equivalent of the Muschelkalk. The author has detected *Orthis Michelini* in a block of this limestone from Wady-Nasb, which leads him to refer it to the Carboniferous epoch, as had already been done by the late Mr. Salter from his interpretation of certain encrinite-stems obtained from it. The author mentioned other fossils obtained from this limestone, and also referred to species of *Lepidodendron* and *Sigillaria* derived from the sandstone of the same locality. He regarded the Adigrat Sandstone of Mr. Blanford as identical with the Nubian Sandstone.

3. " On the Discovery of the Glutton (*Gulo luscus*) in Britain." By W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S.

The author in this paper described a lower jaw of the Glutton, which had been obtained by Messrs. Hughes and Heaton from a cave at Peas Heaton, where it was associated with remains of the Wolf, Bison, Reindeer, Horse, and Cave-Bear. He remarked that he could detect no specific difference between the *Gulo spelæus*, Goldfuss, from Germany, and the living *Gulo luscus*, except that the fossil Carnivore was larger than the living, probably from the comparative leniency of the competition for life in postglacial times. He referred to the distribution of the Glutton in a fossil state, and argued that its association with the Reindeer, the Marmot, and the Musk-sheep, would imply that the postglacial winters were of

arctic severity ; whilst the presence of remains of the Hippopotamus associated with the same group of animals would indicate a hot summer, such as prevails on the lower Volga.

May 24, 1871.—Prof. John Morris, Vice-President, in the Chair.

The following communications were read :—

1. "On the principal Features of the Stratigraphical Distribution of the British Fossil Lamellibranchiata." By J. Logan Lobley, Esq., F.G.S.

In this paper the author showed, by means of diagrammatic tables, what appears to be the present state of our knowledge of the general stratigraphical distribution of the fossil Lamellibranchiata in Britain. As a class, the Lamellibranchs are sparingly repre- sented in the Lower, and more numerously in the Upper Silurian group, and fall off again in the Devonian ; they greatly increase in number in the Carboniferous, become scanty in the Permian and Trias, and attain their maximum development in the Jurassic rocks. They are also largely represented in the Cretaceous and Tertiary series. The stratigraphical distribution of the two great subordinate groups, the Siphonida and the Asiphonida, corresponds generally with that of the class ; the Siphonida predominate over the Asipho- nida in Tertiary formations, whilst the reverse is the case from the Cretaceous series downwards. Nearly all the families of Lamelli- branchs are represented in the Jurassic and Carboniferous rocks, and in the former very largely. The author remarked especially on the great development of the Aviculidæ in Carboniferous times.

2. "Geological Observations on British Guiana." By James G. Sawkins, Esq., F.G.S.

In this paper the author gave a general account of his explora- tions of the geology of British Guiana when engaged in making the Geological Survey of that colony. He described the rocks met with during excursions in the Pomeroon district, along the course of the Cuyuni and Mazuruni rivers, on the Demerara river, on the Essequibo and its tributaries, on the Rupununi river, and among the southern mountains. The rocks exposed consist of granites and metamorphic rocks, overlain by a sandstone, which forms high mountains in the middle part of the colony, and is regarded by the author as probably identical, or nearly identical, with the sandstone stretching through Venezuela and Brazil, and observed by Mr. Darwin in Patagonia.

L. *Intelligence and Miscellaneous Articles.*

ON THE TRANSMISSION OF ELECTRICITY IN LIQUIDS.
ABSTRACT OF A PAPER BY DR. D. MACALUSO*.

IN the first part of this paper the author describes an experimental investigation of the changes in the resistance opposed by a liquid, relatively to the variations of its transverse section, when this section is greater than the surface of the electrodes immersed in the liquid.

* From the *Giornale di Scienze Naturali ed Economiche*, vol. vii. Pa- lermo, 1871. Communicated by R. Gill, Esq.

In the second part he shows, also experimentally, that two currents, proceeding from the same pile and travelling together in a liquid, exercise an influence on each other, both becoming weaker, and that no such influence is exerted between two currents furnished by independent piles—an important fact, which offers a strong analogy to the phenomena of the interference of light, and appears to merit further investigation.

Fechner and Matteucci experimented upon the resistance of liquids to electric currents, but in an incomplete manner and with inexact means of measurement; so that the results obtained by them cannot be considered accurate; and they are at variance in some cases with those given by Dr. Macaluso.

The method followed by the author is similar to that adopted by Becquerel in his researches on the electric conductivity of liquids; but he uses an indicating instrument very much more sensitive and accurate than that of Becquerel—that is, a Weidmann's reflecting galvanometer. He divides the current furnished by the pile into two portions, passing in contrary directions through the two coils of the galvanometer, and regulates the resistance so as to maintain the mirror on the zero of the scale. One of the two circuits is kept constant; the other is variable, and comprehends the liquid and various resistance coils, previously compared with one another and graduated to a common scale. In experimenting, the apparatus was arranged so that the mirror of the galvanometer stood at zero when the liquid resistance was entirely removed by placing the electrodes in contact: the liquid was then interposed, with various lengths and various transverse areas; the mirror of course moved from its zero position, and in order to bring it back a portion of the resistance of the coils had to be removed; this portion of resistance removed was equal to that opposed by the liquid in the circumstances of the experiment. With a view to simplifying the conditions, he adopted as electrodes plates having only one face metallic and naked, the remaining surfaces being varnished; they were of copper, and the liquid adopted was a solution of sulphate of copper.

The length of the liquid conductor was varied by placing the electrodes at different distances apart; and its transverse section was narrowed or widened by means of glass plates placed in a series of grooves made in the bottom and sides of the trough containing the liquid, so as to form a channel of various cross section between the two electrodes.

The author also experimented upon the case of the two electrodes having different widths. And he gives Tables of the resistance opposed by the conducting liquid in the various circumstances; which resistance he has also expressed by means of curves, in which the dimensions of the liquid section are represented by the abscissæ, the resistances by the ordinates. He arrives at the following conclusions.

1. Even when the electrode has only one face in metallic contact with the liquid, the electrical resistance of the liquid depends not only upon the dimensions of the electrodes, but also upon the section of the liquid itself at the sides of the electrodes.

2. That the parts of the liquid having most influence upon its conducting-power are those nearest to the straight line joining the centres of the electrodes.

3. That beyond certain dimensions relatively to certain distances between the electrodes, the cross section of the liquid has no further influence; and that the portion of liquid traversed by the current increases with the distance interposed between the electrodes.

4. That the resistance offered by the liquid, as shown by experiment, does not agree with that calculated according to the law of the resistance of circuits relatively to the length and cross section of the conductor, supposing the conductor to be represented by the liquid prism having the electrodes as bases; and the discrepancy becomes greater as the length of the liquid increases for a given cross section.

From these experiments it follows that when an electric current travels through a liquid whose cross section is much greater than the surface of the electrodes, it tends to diffuse itself laterally. It would be interesting to examine if this happens also when only a section of the liquid is narrowed—if when a diaphragm pierced with a small aperture is interposed, the current diffuses itself in the liquid on either side of it. Dr. Macaluso's experiments show not only that the diffusion takes place, but also that the narrowing of the passage by a diaphragm has comparatively little influence on the resistance, as, by reducing the width of the passage from 14 inches to an inappreciable interval (that remaining between the edges of two plates of glass ground together), the liquid resistance was not quite quadrupled, the resistances in the two cases being as 100 to 378, notwithstanding the very great reduction of the area through which the current had to pass. The experiments showed further that when the conducting liquid was equal in section to the surface of the electrodes, and was made to take the shape of concentric cylindrical shells, its resistance followed the ordinary law of transmission through metallic circuits.

After describing these experiments, Dr. Macaluso, by a theoretical investigation based upon the known law of the resistance of solid and liquid circuits (viz. that such resistance varies inversely with the cross section when, in the case of liquids, the section is equal to the surface of the electrodes), and upon the fact that the resistance diminishes when the cross section of the liquid is made greater for a given surface of the electrodes, comes to the conclusion that two currents travelling side by side in the same liquid exercise an influence upon each other which weakens them both; and gives the following experiments relative to this inquiry:—The electrodes were formed of a series of copper points immersed in the conducting solution; these points could be made to approach or recede from each other at will, remaining always in the same plane, perpendicular to the direction of the current in the liquid, so that the distance between the pair of electrodes remained constant. It was found that upon successively separating the points forming each electrode to a greater distance from each other (that is, upon in-

terposing a greater space between the lines of currents travelling together through the liquid), the resistance diminished in a certain proportion—showing evidently a mutual interference between the various currents emanating from the points and traversing the liquid together. Similar experiments were made with electrodes consisting of a group of small plates with varnished backs ; and it was also found that the resistance of the liquid decreased as the plates composing each electrode were set at a greater distance apart ; such diminution of resistance was more marked when the plates of these compound electrodes were close together, relatively to the distance between the electrodes themselves, and became inappreciable after a certain limit had been attained.

In another experiment a glass plate, immersed in the liquid, was interposed between the electrodes, and the current passed through a rectangular aperture made in the plate ; after observing the resistance, the plate was replaced by another, in which two apertures were made, but of only half the width, so as to divide the current into two branches preserving the same area of passage. This experiment was repeated with apertures of various widths ; and it was found in all cases that, upon splitting the current into two branches without diminishing the transverse section, the resistance diminished, proving that two currents travelling side by side in a liquid weaken each other by mutual interference. But other experiments, made by Marianini, showed that two currents, furnished by two independent piles, traverse a liquid together without interfering with each other. Dr. Macaluso repeated these experiments with greater exactness and with means of noting and measuring the derived currents resulting from the mutual influence of the two circuits, and found Marianini's conclusions to be true. It follows, therefore, that currents travelling together through a liquid do not interfere when furnished by independent piles, and that they do interfere when furnished by the same pile—as in the case of two rays of light, which can be made to interfere only when emanating both from the same source.

From these various researches the author draws the following inferences :—" The facts so far known regarding the passage of electric currents through liquids lead us to imagine that the transmission takes place chiefly by electrolysis. According to Grotthus's theory, which seems the most probable, electrolytic decomposition occurs in such a manner that if a particle M A of a salt is decomposed at one of the poles, for instance the positive pole, such decomposition propagates itself all along a line of particles to the other pole ; the portion M of the molecule remaining free at the positive electrode completes itself by decomposing its neighbouring particle, taking from it and appropriating a portion of matter similar to that of which it has been deprived by the electrode ; the second particle so decomposed acts similarly upon a third, and so on, until at the other electrode the part M is obtained free. And if, instead of a single particle, *n* particles are acted upon by the electrode, the successive decompositions and combinations take place along *n* lines of molecules between the electrodes.

" As the liquid area acted upon by the current becomes greater and greater when its distance from the poles increases, and as, consequently, the lines of chemical action become longer, it appears probable that these lines of action exercise a repulsive influence upon each other ; so that, on their being forced closer together, an increase of resistance or diminution of electric motion follows.

" Electrical motion must therefore be of such a nature that two equal and coincident undulations (that is, undulations proceeding from a single pile) travelling close together, must weaken each other reciprocally, perhaps through a transformation of electrical into thermic motion. Further, there must be a certain difference between the various successive motions, as two equal and coincident undulations proceeding from two independent piles travel together side by side without the least mutual interference."

BOILING-POINTS OF ORGANIC BODIES.

To the Editors of the Philosophical Magazine and Journal.

<div align="right">Graz in Oesterreich (Steiermark),
Heinrichstrasse No. 3,
August 24, 1871.</div>

GENTLEMEN,

You would oblige me very much by inserting the following notice in your Philosophical Magazine. The very interesting treatise of Mr. Burden, " Boiling-points of Organic Bodies " (Phil. Mag. June 1871, Supplementary Number), contains a mistake ; for the author supposes that the velocity of gas-molecules having the temperature of t° C. is to be found by multiplying the velocity at 0° C. by $1 + \dfrac{t}{273}$. But this velocity is really found by multiplying by $\sqrt{1 + \dfrac{t}{273}}$. It thence follows that, at the boiling-point of such substances as have a constant number in the last column of Mr. Burden's Tables, the quotient $\dfrac{(273 + t)^2}{\rho}$ is constant, but not at all the velocity of a molecule. In this formula t signifies the boiling temperature, ρ the specific gravity of vapours.

<div align="right">Yours &c.,
LUDWIG BELTZMANN.</div>

OBSERVATIONS ON THE COLOUR OF FLUORESCENT SOLUTIONS. BY HENRY MORTON, PH.D., PRESIDENT OF THE STEVENS INSTITUTE OF TECHNOLOGY.

As the result of a series of experiments to be presently described, I have come to the curious conclusion that all the familiar fluorescent solutions, such as the tincture of turmeric, of agaric, of chlorophyl, and the solution of nitrate of uranium, emit light of the same colour by fluorescence, namely, blue identical with that developed by acid

salts of quinine. This blue, however, as is well known in the case of quinine, is not of a single tint or refrangibility, but yields a continuous spectrum in which the more refrangible rays predominate.

My attention was first drawn to the subject by observing that a specimen of mixed asphalt, which is here largely used in the preparation of pavements, yielded a light-yellow solution with alcohol which fluoresced blue, and an orange solution with turpentine which fluoresced green. It at once occurred to me that the green colour was simply due to the absorptive action of the coloured solution, and not to the development of green rays. Examined with the spectroscope, the seemingly green fluorescence showed no increase in the green or yellow part of the spectrum as compared with the blue fluorescence, but only an absorption of the red and violet ends. When, however, a piece of fluorescing canary glass or solid nitrate of uranium was examined, the green light was (as is well known) largely augmented. I also found that when, by filtration through animal charcoal, the solution in turpentine was reduced in colour, the green tint of the fluorescence disappeared in a corresponding degree. This alone, however, would have proved nothing, as a green fluorescing matter might have been absorbed by the charcoal; but in connexion with the spectroscopic result it was of interest.

I next took up for examination the tincture of turmeric. This is set down in standard works, such as those of Du Moncel and Becquerel, as fluorescing red. This solution, when concentrated, has a rich orange-red colour; and the jacket of a Geissler tube being filled with it, all the light reaching the eye from the electric discharge within is of a deep orange or red colour. If, however, the solution is simply diluted until its colour is reduced to a rich yellow, the fluorescence appears green. The same result follows from filtration through bone-black, with a marked increase in the amount of fluorescence visible as the light-absorbing colouring-matter is removed. By continuing the decoloration until the liquid is colourless or of a very light tint, its fluorescence is distinctly blue.

The results with the spectroscope when it was applied to this substance were the same as with the solution of asphalt. Such also is the case with tincture of chlorophyl, which, when fresh and green, gives apparently a green light, and when old and brown a grey colour.

Finally, I took up the nitrate of uranium, about which such contradictory statements have been published. This salt in its solid state gives a brilliant green fluorescence whose spectrum is figured by Becquerel and abounds in green rays; but in solution it gives a very feeble fluorescence, far inferior to that of turmeric, and of no more green tint than would be due to its yellow colour. So in fact says also the spectroscope.

From these results it would seem that the molecules of fluorescent bodies *in solution* are not capable of restricting their vibrations to limited ranges, but move at rates corresponding with all refrangibilities, having simply an excess of the higher ones, though the same substances in the solid state may act quite differently, as in the case

of nitrate of uranium, and possibly the fluorescent material in the asphalt, which may be related to the solid hydrocarbon fluorescing green which Becquerel mentions (*La Lumière,* tome i. p. 382).

In this general connexion let me mention that I have observed that while the acid salts of quinine generally are fluorescent, the chloride is not, and that hydrochloric acid will decompose the acid sulphate so as to destroy its fluorescence.

There are several other points in connexion with this and the foregoing subject, which I must leave for a subsequent discussion.

P.S.—August 1. I have just obtained results with turmeric which seem to indicate that its fluorescence is due to the presence of a substance not yet observed, soluble in water, and without any colour.—Silliman's *American Journal,* September, 1871.

ON THE SPECTRA OF THE SIMPLE GASES.
BY M. A.-J. ÅNGSTRÖM.

In the *Recherches sur le Spectre solaire* which I published in 1868, I already announced that the spectrum-observations to which I had devoted myself had not convinced me of the correctness of the opinion of Plücker that one and the same gas, in the state of incandescence, could give spectra varying with its temperature. I rather believed that in the appearance of the spectra a modification may be observed which consists in the elevation of the temperature bringing about a greater abundance of lines, and that the relative luminous intensity of these lines may also undergo some changes, but that nevertheless the spectrum preserves its character unaltered. It is true that in disruptive discharges it happens, when the tension of the gas is increasing, that the spectral lines spread, and even end by uniting so as to form a continuous spectrum ; but even then one cannot say that the result is a new spectrum.

Several distinguished physicists, however, are of the opposite opinion ; and probably the researches of M. Wüllner (according to which hydrogen would have no less than four spectra, oxygen three) have strengthened this conviction in the minds of many savants. M. Dubrunfaut has expressed his doubts of the correctness of these results : he remarks that the multiple spectra of oxygen and hydrogen may be due to nitrogen or mercury-vapour introduced by the pump into the tubes. M. Wüllner, however, has shown (*Comptes Rendus,* Jan. 17, 1870) that this explanation is inadmissible.

Yet, as the question of the multiple spectra of the gases is a vital one for spectral analysis, and in this light M. Wüllner's observations are truly important, perchance the following analysis of the phenomena observed will not be without interest. Permit me to commence with a preliminary remark. According to the experience acquired, at least by me, the results obtained concerning the spectra of the gases are not absolutely sure when the rarefaction is carried to its utmost limits. In proof of this I cite the following fact :— On one occasion, when I rarefied as much as possible, by means of a mercurial pump, the atmospheric air in a Geissler's tube, at the same time causing the discharge of a Ruhmhorff coil to pass in the

tube, I obtained in succession the following spectra :—1, the ordinary air-spectrum ; 2, the fluted spectrum of nitrogen : 3, that of carbonic oxide ; 4, when the rarefaction was at its maximum, the lines of sodium and chlorine. If to this we add that when a mercurial pump is employed the mercury-lines may present themselves, just as those of sulphur may when sulphuric acid is used to dry the gas, the result may easily be a multiplicity of spectra which it would be wrong to attribute to one and the same gas.

As far as I know, I was the first to observe (in 1853) the spectrum of hydrogen. Using on that occasion a Leyden jar to produce incandescence of that gas, which was at the pressure of the atmosphere, I obtained a spectrum consisting of an intense line in C not clearly limited, and two maxima of light in F and G ; the third maximum, in *h*, was only observed later. Afterwards Plücker found that by operating with rarefied hydrogen a spectrum is obtained with clearly determined lines. It may thus be regarded as a fact long known, that the spectrum-lines of hydrogen spread when the discharge becomes disruptive, and that they end, when the tension of the gas is augmented, by forming a continuous spectrum. M. Wüllner's spectrum No. 4, then, is only the ordinary spectrum of hydrogen.

Plücker was the first who indicated a second spectrum for hydrogen, principally characterized by a multitude of lines on both sides of D and towards C. This spectrum generally appears simultaneously with the preceding, but is distinguished from it by several important characters. By causing the discharge of a Ruhmkorff coil to pass in a Geissler's tube containing rarefied hydrogen, in a revolving mirror two images of the incandescent gas are obtained, which correspond to the two spectra : one of them appears as an isolated line, indicating that the light there is of very short duration ; the other, on the contrary, widens into a zone traversed horizontally by striæ alternately bright and dark. It is necessary, in this experiment, to regard the Geissler's tube and the axis of rotation of the mirror as placed vertically. The duration of the last image, in one experiment, was from 5 to 6 thousandths of a second *.

This image disappears immediately the discharge is made disruptive by the addition of a condenser. This property, as well as the stratification of the light which accompanies it, indicates that we have here a combination of hydrogen, either with itself or a foreign body ; the latter is the most probable. M. Berthelot has published, in the *Comptes Rendus*, some observations on a spectrum which he obtained by means of a combination of hydrogen and benzole. He submits that this spectrum belongs to *acetylene*, and that it has not been previously observed. This, however, is not the case ; having repeated M. Berthelot's experiments with some benzole, I ascertained that the spectrum obtained is no other than M. Wüllner's hydrogen-spectrum No. 2. Still, if (as M. Berthelot has shown)

* To determine the duration of an image, I used M. Kœnig's flames. By projecting simultaneously on the revolving mirror the flame agitated by the pipe, we have a scale by means of which we can easily determine the duration of the luminous phenomena when that duration is very short.

acetylene mixed with a sufficient quantity of hydrogen remains unaltered in a Geissler's tube, so that a decomposition, if produced, is always accompanied by a corresponding combination, there is nothing to prevent us from admitting that the hydrogen-spectrum No. 2 belongs to *acetylene.*

I pass now to the third of the spectra which M. Wüllner thinks he has found for hydrogen. This spectrum, which would be quite new if it belonged to the gas in question, is in all probability only that of *sulphur.* This, I believe, is most positively demonstrated by the following Table, which contains the wave-lengths of the vapour of sulphur and those determined by M. Wüllner for this third spectrum of hydrogen. The differences met with in the two series are easily explained by this consideration—that the wave-lengths cited, both for sulphur and for M. Wüllner's spectrum, have not all the accuracy desirable.

Supposed Spectrum of Hydrogen.		Sulphur-Spectrum.
1. Group of three lines; the middle line	5647	5671 5645 5613
2. Group of three lines; the middle line	5469	5474 5451 5432
3. Group of two lines; the second line......	5334	5345 5322
4. Group of two series; the first line........	5221	5207 5191
5. Group of three lines; the middle line	5015	5027 5013 4994
6. Group of more than six lines; the middle line.	4930	4926

I persist, therefore, in the opinion that hydrogen has only one spectrum—the one found in the light of the sun and of the stars.

Beside the known spectrum of oxygen, M. Wüllner has observed two new ones, which, for brevity, we will designate by the numbers 2 and 3. According to the description given of No. 2, it is composed principally of four shaded bands with clean edges on the side towards the red field of the spectrum. In order to obtain a more exact idea of the position of the bands, I constructed the spectrum itself with the aid of the minima of deviation given by M. Wüllner. I found that it presented much analogy with the spectrum of the oxide of carbon. I afterwards determined the wave-lengths of the four bands, by construction and with the aid of the wave-lengths already calculated by M. Wüllner. The following Table gives the values obtained, and the wave-lengths for the oxide of carbon :—

Supposed Spectrum of Oxygen, No. 2.			Spectrum of the oxide of carbon.
1. First band	......	5620	5609
2. Second band	......	5203	5196
3. Third band	......	4835	4834
4. Fourth band	......	4506	4510

As is seen, the agreement is perfectly satisfactory; let us add that it is equally so in the details, such as the presence of a fainter shaded line between 1 and 2, and of two shaded bands in the red field. Hence it is impossible to doubt that the spectrum No. 2 belongs to the *oxide of carbon*.

There now remains spectrum No. 3. This no more than the other belongs to oxygen. Perhaps some lines of the known spectrum of oxygen are found in it; but most of the lines belong to *chlorine*. This assertion is put beyond doubt by the following Table, which contains the wave-lengths of the chlorine-spectrum and those of spectrum No. 3.

Supposed Spectrum of Oxygen, No. 3.

1. A large group	the first line ..	5461	5460	} Chlorine.
	the last line ..	5404	5399	
2. A large group	the first line ..	5215	5213	
	the middle line, very bright.. }	5152		} Oxygen?
	the last line ..	5090		
3. Group of six series .	the first	4938	4940	
	the last	4893	4895	
4. A large group .. .	the last line ..	4805	{ 4820 4808 4793	} Chlorine.
5. Group of three lines	the first	4652	4647	
	the second....	4644	4642	Oxygen.
	the third	4637	4630	Chlorine.
6. A violet line		4418	4417	Oxygen.
7. A violet line		4621	?	

The result of the above examination will therefore be, that we know not yet any other oxygen-spectrum than the one observed by me in 1853, and which has subsequently been studied with great care by Plücker.

I ask permission to add a few words on the action exerted by magnetism on the spectra of gases; these considerations have an intimate connexion with what precedes. Under the influence of this action the spectrum assumes, according to M. Trève, a quite different aspect; so that we should be able to produce not merely by a rise of temperature, but also by magnetism, the multiple spectra which, in the opinion of divers savants, present themselves with gases. This is correct in several respects; but the explanation of the phenomenon appears to me to be different from that which has been given of it. In fact the modification in the appearance of the spectra depends simply on this—that *the action of magnetism causes, at the incandescent state, the occurrence of other substances or other combinations.* In certain cases the effect of magnetism may be compared almost to that which is produced by the addition of a condenser to the Ruhmkorff coil; but magnetism appears also to exert a sort of chemical action, obstructing the production of certain combinations, and facilitating the production of others.

Thus a Geissler's tube has given, between the poles of an electromagnet, the ordinary spectrum of carburetted hydrogen, whereas, without the intervention of magnetism, it gave the carbonic-oxide spectrum without the lines of hydrogen being visible.

In another tube, filled with hydrogen obtained by the decomposition of water and dried with sulphuric acid, which gave Plücker's two hydrogen-spectra, under the influence of magnetism there appeared those sulphur-lines which M. Wüllner has regarded as forming the hydrogen-spectrum No. 3, while the spectrum of carbonic oxide was shown on the polar wires.

It would doubtless be premature to endeavour to formulate a law according to which these changes take place; but a positive fact is, that they do not appear to introduce any new spectrum peculiar to the action of the magnetic forces.—*Comptes Rendus*, August 7, 1871.

ON THE TESTIMONY OF THE SPECTROSCOPE TO THE TRUTH OF THE NEBULAR HYPOTHESIS. BY PROFESSOR DANIEL KIRKWOOD, OF BLOOMINGTON, INDIANA.

In March 1846 the partial resolution of the great nebula in Orion was announced by Lord Rosse. In September of the following year the late Professor W. C. Bond, of Harvard University, stated, in confirmation of this interesting discovery, that the part of the nebula about the Trapezium " was resolved into bright points of light " by the great refractor of Cambridge. " It should be borne in mind," continued Professor Bond, " that this nebula and that of Andromeda have been the last stronghold of the nebular theory— that is, the idea, first thrown out by the elder Herschel, of masses of nebulous matter in process of condensation into systems."

These grand achievements were regarded by the majority of astronomers as fatal to the claims of the nebular hypothesis. It is not to be denied, however, that this celebrated theory has more than recovered from the shock which it then received, that it has, in fact, been materially strengthened by the researches and discoveries of the last twenty years. The truth of this remark is strikingly exemplified by the revelations of the spectroscope. The man who at the middle of the nineteenth century would have been bold enough to predict the discovery of the physical constitution of the heavenly bodies, or the determination of the elements of which they are composed, would have been generally deemed a scientific enthusiast. This, however, and more than this, has been actually accomplished. In the hands of Huggins, Secchi, Young, and others the *spectroscope*, that marvel of modern science, has yielded satisfactory testimony, not only in regard to such stars as are reached by our unassisted vision, but even respecting the telescopic nebulæ, apparently on the outskirts of the visible creation. A detailed account of these wonderful achievements would not comport with our present purpose. Such *results*, however, as bear directly upon the theory of Laplace will be briefly noted.

1. The ring nebula in Lyra, the Dumbbell nebula, the great ne-
bula in Orion, and others which might be named, are not, as was
but recently believed, extremely remote sidereal clusters ; but *their
light undoubtedly emanates from matter in a gaseous form.*

2. " According to Lord Rosse and Professor Bond, the brighter
parts near the trapezium [in the nebula of Orion] consist of cluster-
ing stars. If this be the true appearance of the nebula under great
telescopic power, then these discrete points of light must indicate
separate and probably denser portions of the gas, and that the whole
nebula is to be regarded rather as a system of gaseous bodies than
as an unbroken vaporous mass " *.

3. Progressive changes in the physical condition of certain nebula
are clearly indicated by the fact that nuclei have been established
which, as shown by their spectra, are not wholly gaseous, but have
passed, at least partially, to the solid or liquid form.

4. The spectroscopic analysis of the light of several comets reveals
a constitution similar to that of the gaseous nebulæ.

The spectroscope, then, has *demonstrated* the present existence
of immense nebulous masses, such as that from which Laplace sup-
posed the solar system to have been derived. It has shown, more-
over, a progressive change in their physical structure, in accordance
with the views of the same astronomer. In short, the evidence
afforded by spectrum analysis in favour of the nebular hypothesis is
cumulative, and of itself sufficient to give this celebrated theory a
high degree of probability.—Silliman's *American Journal*, September
1871.

SOLID CRUST OF THE EARTH.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Will you be so good as to print this at the end of your next
Number after receiving it, in order to correct the following *errata* in
my letter in your August Number, which has just reached Calcutta?

Page 98, line 13, *for* column 1 *read* p. 400, column 1.
— —, — 36, *after* fluid *insert* and of the fluid on itself.
— 99, — 5, for *a* ead a.
— —, — 14, *for* colatitude *read* codeclination.
— 100, — 4, *for* $\cos (l - \theta')$ *read* $\sin (\theta' - l)$.
— —, — 21, dV should be added for the attraction of the fluid
on itself. But V immediately disappears again from the moments,
as the centrifugal force does, owing to the symmetry of figure ; and
the subsequent calculation and reasoning are not affected.

Page 100, line 29, *for* P_1 *read* $c P_1$.

I am,
Yours faithfully,
J. H. PRATT.

Calcutta, September 4, 1871.

* Monthly Notices of the Royal Astronomical Society, vol. xxv. p. 156.

Fig 1
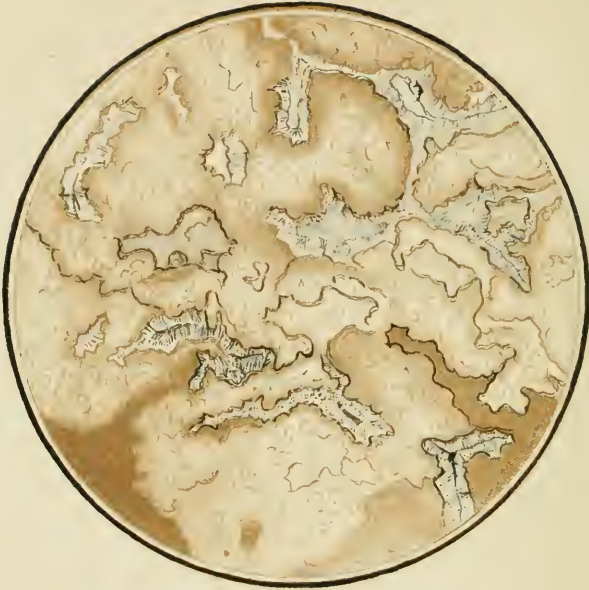
X.75

Fig.2.

X.30.

A.C.P del.   Mintern Bros. lith.

LI. *On the Connexion of certain Phenomena with the Origin of Mineral Veins.* By J. ARTHUR PHILLIPS, F.C.S., M. Inst. C.E., &c.*

[With a Plate.]

THE origin of mineral veins is a subject which has long occupied the attention of writers on natural history ; and numerous theories have at different times been framed with the object of explaining the nature of the causes by which they are supposed to have been produced.

An enumeration and classification of those which had been brought forward previously to 1838 is given by Baron von Herder in his work on the Meissen adit, published in that year.

This may be briefly summarized as follows :—

1st. *Theory of contemporaneous formation.*—According to this, lodes are not mineral matter occupying previously existing fissures, but were formed either contemporaneously with the enclosing rock, or were subsequently produced by metamorphic action.

2nd. *Theory of lateral secretion.*—Lodes are fissures filled by various mineral substances derived from the rocks enclosing them.

3rd. *Theory of descension,* by which veins are explained as being produced by the filling of fissures by materials introduced from above.

4th. *Theory of ascension.*—This teaches that veins are the result of deposits of mineral substances which have been introduced into fissures from below.

Four distinct modifications of the last theory may be divided into as many sub-classes.

* Communicated by the Author,

a. Infiltration.—The material was introduced by aqueous solution.

b. Aqueous vapours.—The fissures were filled with mineral matter introduced by the agency of steam.

c. Sublimation.—The substances constituting mineral veins were introduced in a gaseous condition by sublimation.

d. Injection.—The materials forming veins have, according to this view of the question, been introduced in a state of igneous fusion.

The formation of veins has been the subject of much speculation since the date above referred to ; and various authors have either advanced new theories, or have advocated more or less important modifications of those before promulgated.

Among the most worthy of consideration may be cited the researches of Mr. R.W. Fox, who, after ascertaining the existence of electric currents in many of the metalliferous veins of Cornwall, suggested the probability of this force having acted on the various metallic sulphides and chlorides, dissolved in the hot water traversing fissures, in such a way as to determine the peculiar mode of their distribution. He has also endeavoured to account for the prevalence of an east and west direction in the principal Cornish lodes by their position at right angles to the earth's magnetism.

Weighty objections to this theory, however, have been pointed out by W. J. Henwood, as well as by other experienced miners; and observed facts would indicate that the general direction of veins, in different mining-districts, varies so entirely, that it probably depends rather on lines of fracture than on the action of voltaic currents. In fact Von Beust, in his criticism on Werner's theory (1840), appears to have conclusively demonstrated that the majority of lode-fissures have been produced by volcanic or plutonic agency.

It is well known that mineral veins are usually found in regions in which igneous rocks are abundant, and in situations where they have burst through crystalline or other stratified deposits. Lodes also more frequently occur in rock-formations of great age than in recent ones; and certain classes of them (as, for instance, those of oxide of tin) are only found in the oldest rocks.

Sir Charles Lyell, when speaking of chemical deposits in mineral veins, remarks, " We know that the rents in which ores abound extend downwards to vast depths, where the temperature of the interior of the earth is more elevated. We also know that mineral veins are most metalliferous near the contact of plutonic and stratified formations, especially where the former send veins into the latter—a circumstance which indicates an original proxi-

mity of veins, at their inferior extremity, to igneous and heated rocks. It is moreover acknowledged that even those mineral and thermal springs which, in the present state of the globe, are far from volcanoes, are nevertheless observed to burst out along great lines of upheaval and dislocation of rocks "*.

It is generally admitted by geologists that igneous causes have been in active operation in all past ages of the world. They have been constantly changing their areas of activity on the earth's surface; and those districts which are now great centres of volcanic action were, at remote geological epochs, in a state of perfect tranquillity; on the other hand, districts in which violent eruptions took place at former periods have often been entirely free from volcanic eruptions during later geological times. It would appear that the last expiring efforts of volcanic action frequently manifest themselves in the form of geysers or boiling springs, which, in addition to pouring forth steam and water holding various salts and minerals in solution, give off carbonic acid, sulphuretted hydrogen, and other gases.

Hot and boiling springs are exceedingly numerous in some of the mining regions of the Pacific States of North America, and present phenomena possessing the highest degree of interest to the geologist†.

The Steamboat Springs, seven miles north-west of Virginia city in the state of Nevada, are probably the most remarkable of those yet discovered. They are situated at a height of 5000 feet above the level of the sea, near the foot of the eastern declivity of the Sierra. In this locality the granite is traversed by numerous parallel fissures, which either give out highly heated water or simply eject steam. The principal group of crevices, of which the direction is nearly north and south, comprises five longitudinal openings, extending nearly in a straight line for a distance of more than 1160 yards. These are often full of boiling water, which sometimes overflows and escapes in the form of a considerable rivulet, whilst at others it does not reach the surface, although violent ebullition is heard to be taking place below. These waters are slightly alkaline, and contain sodium carbonate, sodium sulphate, and common salt. There is along the whole line an escape of carbonic acid, sulphuretted hydrogen being also evolved from certain points in notable quantities.

Silica, sulphur, and oxide of iron are deposited; and the fissures are lined on either side (sometimes to the thickness of several feet) by incrustations of more or less hydrated silica. This is con-

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* Elements of Geology, p. 766.
† " Notes on the Chemical Geology of the Gold-fields of California, by J. Arthur Phillips," Phil. Mag. November 1868.

stantly accumulating on the sides, whilst a longitudinal central crevice affords a passage for the escape of steam and boiling water. This silica forms a series of semi-crystalline bands parallel with the walls of the fissure, and often presents the comby appearance peculiar to the beds of deposition so frequently observed in mineral veins.

At some distance to the west of the line of fissures now in a state of activity, is another group of longitudinal siliceous deposits, also presenting a central crevice, from which steam and carbonic acid still escape, although no longer traversed by hot water. The silica from this locality contains oxides of iron and manganese, together with traces of copper, and minute crystals of iron pyrites. It has also been stated to be sometimes slightly auriferous; but I was unable to detect this metal in any of the specimens selected for examination.

The granite in which the fissures occur is covered, both on the eastern and western sides of the valley, by a compact basalt.

On analysis the deposited silica was found to contain from 4·72 to 6 per cent. of water; and on being boiled, in a finely divided state, with a strong solution of caustic potassa, 6·68 per cent. of its weight was dissolved.

Thin sections of this substance examined under the microscope show the reniform structure of chalcedony, with patches of amorphous silica, stains of oxide of iron, and geodes of crystalline quartz. Some few specimens also contain minute crystals of pyrites. An attempt to give an idea of the appearance of a section of the deposit from Steamboat Springs, when magnified 75 diameters, is made in Plate I. fig. 1. The crystals of quartz lining cavities filled with amorphous silica become very distinct when examined by polarized light; but the chalcedonious markings then almost entirely disappear.

At the "Sulphur Bank" on the shores of Clear Lake, California, is a solfatara some six or seven acres in extent, where a much decomposed volcanic rock is traversed by innumerable fissures, from which steam, together with carbonic and boracic acids, is continually issuing. Sulphur is deposited on the sides of the crevices; and gelatinous silica is found coating chalcedony and opalescent silica in various stages of formation, from the gelatinous state to that of the hardest opal. This indurated silica is sometimes nearly colourless, but is more frequently permeated by cinnabar and iron pyrites, or blackened by a tarry hydrocarbon. Cinnabar is also found in striæ, and occasionally in veins, as well as in concretionary masses of considerable size.

Sections of chalcedony and semi-opal from this place, when examined under the microscope, are often found to enclose crys-

tals of pyrites, together with crystalline cinnabar, although the latter mineral has generally been deposited in an amorphous state*. A specimen of chalcedony taken from one of the fis- sures in the sulphur bank, which on being first broken was ex- teriorly so soft as readily to receive an impression of the nail, had on reaching this country become hardened, and had assumed the ordinary characteristics of that mineral. Thin sections of this specimen show a structure resembling fine-grained fortification agate, and are traversed by numerous fissures filled by opaque oxide of iron. Fig 2 is intended to show the appearance of a section of this substance magnified 30 diameters. When ex- amined between crossed prisms, brilliant colours are obtained, and the crystalline structure becomes exceedingly distinct.

The vein-matter of lodes on the Pacific Coast of North Ame- rica has generally so many characteristics in common with de- posits produced by solfatara action, that various geologists, who have examined that region, have arrived at the conclusion that the two are the result of similar causes.

Von Richthofen remarks that "the process which immediately follows the opening of fissures at or near active volcanoes, is the violent emission of steam. A crevice in this state is called a *sol- fatara*. It has been proved by Bunsen for the volcanoes of Ice- land, by Boussingault for those of the South American Andes, by St. Claire-Deville for those of the Canary Islands, and by myself for the tertiary volcanoes of Hungary and Transylvania, that every solfatara, in the course of time, passes through two stages, in the first of which the steam is accompanied by gaseous combinations of fluorine and chlorine, in the second by those of sulphur, while a third one is ordinarily marked by the emission of carbonic acid and combinations of hydrogen and carbon; at which time the term solfatara is no longer applicable. We have in the elements evolved during the first two periods, all the con- ditions required for filling the Comstock fissure with such sub- stances as those of which the vein is composed "†.

W. P. Blake describes the lodes at Bodie Bluff, nine miles west of the town of Aurora, as being of a character favourable for the production of gold, and then goes on to say :—" They are all alike in this respect, and doubtless had the same origin. The quartz, instead of being a solid homogeneous mass, is formed in thin layers or coats, one over the other, like sheets of paper or pasteboard, with irregular thin seams or openings between. This structure, with other peculiarities, indicates that the veins were deposited gradually in the fissures by thermal springs,

* *Jahrbuch für Mineralogie,* &c., 1871, vol. iii. p. 291.
† On the Comstock Lode, its character, and the probable mode of its continuance in depth, p. 47. San Francisco : 1865.

similar perhaps to those now existing at various points along the eastern base of the Sierra Nevada, as, for example, at Steamboat Springs, Washoe "*.

Daubrée observes that veins can have been but very rarely filled either by fusion or sublimation, but that, on the contrary, the materials of which they are composed have been deposited from solution in highly heated waters†.

The late M. de Senarmont has shown that the principal minerals found in veins may be produced in a crystalline form by the aid of water heated to temperatures varying from 130° to 300° Centigrade. Among the minerals which were thus obtained may be enumerated quartz, spathose iron, the carbonates of manganese and zinc, sulphate of baryta, sulphide of antimony, mispickel, red silver ore, &c.‡

In the present state of our knowledge we are unable to explain all the various phenomena which have been observed in connexion with the origin, composition, structure, and mineral constitution of veins ; but a careful consideration of ascertained facts would appear to lead to certain general conclusions, forming a sort of skeleton map, of which the details remain to be filled in by the aid of further research.

First. Metalliferous lodes are more numerous and productive in the vicinity of igneous rocks than elsewhere.

Secondly. There is abundant evidence of volcanic eruptions having taken place during all periods of geological time§.

Thirdly. Solfatara action and thermal springs are often the latest active evidences of volcanic disturbance.

Lastly. Crystalline quartz, iron pyrites, sulphide of mercury, and various other minerals are at the present time being deposited by solfatara action, in veins possessing many of the characteristics of ordinary lodes.

* Report on the Property of the Empire Gold and Silver Mining Company, by Professor B. Silliman, Jun., and W. P. Blake, p. 37. New York : 1864.

† *Rapport sur les progrès de la Géologie Expérimentale*, p. 63. Paris, 1867.

‡ " Expériences sur la formation artificielle, par voie humide, de quelques espèces minérales qui ont pu se former dans les sources thermales sous l'action combinée de la chaleur et de la pression," *Annales de Chimie et de Physique*, vol. xxviii. p. 693. " Expériences sur la formation de quelques minéraux, par voie humide, dans les gîtes métallifères concrétionnés," ibidem. vol. xxxii. (1851). ·

§ Recent microscopical investigations have also shown that eruptive rocks of very different geological ages completely agree in composition, texture, and mode of occurrence. S. Allport, in a paper " On the Relative Ages of Igneous Rocks," published in the Geological Magazine (October 1871), says, " I have abundant evidence that melaphyres of undoubted Carboniferous age, and basalts of Tertiary age, have not only the same mineral constitution, but also that both present the same structural variety."

Is it then unreasonable to suppose that true mineral veins have originally been fissures, often produced by volcanic disturbances, which have subsequently become filled during the ensuing periods of solfataras and hot springs ? This view of the subject would appear to indicate the nature of the connexion existing between such veins and eruptive rocks, and also to explain why they should generally be of more frequent occurrence in the older rocks than in formations of comparatively recent date.

Carefully conducted analyses, executed on large quantities of the waters issuing from active solfataras and thermal springs, might probably afford valuable information on this subject; but it must not be forgotten, as observed by M. E. de Beaumont, that we cannot expect the contents of hot springs and mineral veins to be identical. The more insoluble of the bodies in solution, both simple and compound, will be deposited in proportion as the temperature and pressure decrease; the more soluble only being discharged in the waters issuing from the surface*.

The occurrence in lodes of minerals exhibiting pseudomorphic forms, apparently produced by their deposition in moulds left by the removal of crystals of other substances, and the presence in drusy cavities of stalactites of calcite, quartz, pyrites, &c., indicate that a partial decomposition and re-arrangement of some of their constituents has been effected by the action of water at comparatively low temperatures. There can also be little doubt that fissures and cavities have sometimes been filled by infiltration from the enclosing rocks, as well as by the percolation of meteoric waters from the surface. The operation of these agencies is perhaps, in most instances, extremely slow, although, according to R. B. Smyth, even gold, under certain conditions, may be deposited in appreciable quantities within comparatively short periods. This author states that, in the gold-fields of Victoria, pieces of highly mineralized fossil wood, taken from the deeper workings, as well as timber used for supporting galleries, which had remained in the mine for some years, have exhibited, under the microscope, particles of gold adhering to and intermixed with crystals of iron pyrites, all through the central parts of the wood†. This is confirmed by Mr. Ulrich, who says that in the gold-drifts pyrites is often found incrusting or replacing roots and driftwood, and that samples, assayed by Messrs. Daintree, Latta, and Newbery, have yielded amounts of gold varying from a few pennyweights to several ounces per ton. According to Mr. H. A. Thompson, a specimen of pyrites from the centre of an old tree-trunk gave by assay above 30 oz. of gold per ton‡.

* *Bulletin,* vol. iv. p. 1278.

† The Gold-fields and Mineral Districts of Victoria, by R. Brough Smyth, p. 74.

‡ Notes on the Physical Geography, Geology, and Mineralogy of Vic-

On examining pyrites forming the substance of fossilized trees found in the deep diggings at French Corral, California, I was unable to detect any decided traces of gold; but assays of several specimens of the cementing pyrites of an auriferous deposit of tertiary age, and probably belonging to the later Pliocene period, invariably afforded a small amount of that metal. A microscopical examination of the gold in this pyrites showed that the fragments were frequently angular, and apparently unwaterworn.

In order to ascertain whether waters traversing mineral veins contain an appreciable quantity of any of the constituents of the lodes through which they pass, I subjected three specimens, obtained from deep Cornish mines, to careful analysis. I am indebted for the samples operated on to Mr. Francis Oats, of Botallack, a member of the Miners' Association of Cornwall, who in each case collected about six gallons of the water issuing from the bottom of a level, where the lode beneath remained intact, in clean and carefully stoppered glass bottles.

Water from Balleswidden Mine.—This was collected from the bottom of a sink below the 50-fathom level on the " Pye Lode," which is one of a series of veins having a south-easterly direction occurring in granite, and which are chiefly composed of quartz, mica, schorl, and oxide of tin. The nearest known junction of granite with clay-slate is at a distance of about one and a half mile from the point from which the water was collected.

The following are the results, in grains per gallon and grammes per litre, obtained by analysis.

Specific gravity not taken.   Total solid contents 13·59 grains per gallon.

	Grains per gallon.	Gramme per litre.
Chlorine	3·59	·0512
Sulphuric acid (SO^4).	4·80	·0685
Silica	·70	·0100
Alumina	trace	trace
Lime	2·80	·0400
Magnesia	trace	trace
Iron*	·08	·0011
Alkaline chlorides .	6·10	·0870
Potassium	·49	·0070
Sodium	2·02	·0288
Carbonic acid . .	trace	trace

toria, by Alfred R. C. Selwyn and George H. F. Ulrich, p. 56.   Melbourne: 1866.

* By potassium dichromate.

The foregoing results may be rendered thus* :—

	Grains per gallon.	Gramme per litre.
Calcium sulphate	6·80	·0970
Sodium chloride	5·15	·0735
Potassium chloride	·95	·0135
Aluminium chloride.	trace	trace
Ferrous carbonate	·16	·0022
Silica	·70	·0100
Magnesium chloride.	trace	trace
Carbonic acid	trace	trace
Total by addition of constituents .	13·76	·1962
Total found directly	13·59	·1938

Water from Botallack, No. 1.—This was issuing from whole ground at the 205-fathom level, "Higher Mine," in granite. It is the deepest part of the mine in that portion of the workings; and the vein is remarkably free from the presence of sulphides, but yields tin ore of great purity. The following results were obtained by analysis :—

Specific gravity = 1·0006.   Total solid contents 42·19 grains per gallon.

	Grains per gallon.	Gramme per litre.
Chlorine	9·78	·1394
Sulphuric acid . .	15·65	·2232
Silica	·70	·0100
Alumina	trace	trace
Lime	9·10	·1297
Magnesia	trace	trace
Iron	1·40	·0199
Arsenic	trace	trace
Alkaline chlorides .	16·50	·2353
Potassium	·94	·0134
Sodium	5·78	·0824
Carbonic acid . .	not estimated	,,

* As the state of combination in which the various substances present in mineral waters exist cannot be accurately determined, the system of grouping, adopted in this and the following Tables of a similar nature, must, to some extent, be regarded as arbitrary.

These results may be expressed as follows :—

	Grains per gallon.	Gramme per litre.
Calcium sulphate	22·14	·3157
Sodium chloride	14·70	·2096
Potassium chloride	1·80	·0257
,, arseniate	trace	trace
Magnesium chloride	trace	trace
Aluminium chloride	trace	trace
Ferrous carbonate	2·90	·0414
Silica	·70	·0100
Total by addition of constituents .	42·24	·6024
Total found directly	42·19	·6017

Water from Botallack, No. 2.—This water was collected from the 245-fathom level, under the sea, in the "Crown Mine," from the deepest part of the workings. The lode is enclosed in a magnesian clay-slate, and chiefly consists of quartz spotted with copper pyrites, arsenical pyrites, oxide of tin, galena, blende, and calcite.

The results obtained by analysis were as follows :—

Specific gravity = 1·0105.   Total solid contents 1003 grains per gallon.

	Grains per gallon.	Grammes per litre.
Chlorine . . .	440·17	6·2778
Iodine	trace	trace
Bromine . . .	trace	trace
Sulphuric acid . .	85·32	1·2168
Silica	trace	trace
Alumina . . .	·45	·0064
Lime	140·02	1·9970
Magnesia . . .	15·30	·2182
Iron	0·70	·0100
Alkaline chlorides .	684·00	9·7556
Potassium . . .	9·76	·1392
Lithium . . .	·84	·0119
Sodium	259·80	3·7054
Carbonic acid . .	155·05	2·2114

The foregoing results may be thus tabulated :—

	Grains per gallon.	Grammes per litre.
Calcium sulphate	120·87	1·7240
Sodium chloride	660·80	9·4247
Potassium chloride . . .	16·82	·2400
,, iodide	trace	trace
,, bromide	trace	trace
Lithium chloride	5·10	·0727
Magnesium chloride . . .	37·05	·5284
Aluminium chloride	·86	·0122
Calcium carbonate	161·16	2·2985
Ferrous carbonate	1·45	·0207
Silica	trace	trace
Total by addition of constituents.	1004·11	14·3212
Total found directly . . .	1003·00	14·3047
Excess of carbonic acid . .	83·59	1·1923

It will be observed that the amount of soluble matter in the first two specimens is small, and that the constituents do not differ materially from those which might be expected to occur in ordinary well-water from the same district.   The third sample evidently consists of a mixture of sea-water with water derived from other sources.

The solid contents, however, are considerably less than in sea-water, and consequently, in order to institute an approximate comparison between them, we will suppose sea-water to have been diluted with distilled water until the total amount of fixed constituents has been reduced to that contained in the water from Botallack.

For the purposes of this comparison I have selected an analysis of water from the Irish Sea by T. E. Thorpe and E. H. Morton, in which the fixed constituents amounted to 33·8385 grammes per litre[*].   The water from Botallack afforded 14·3047 grammes of solid matter per litre; and therefore, if we multiply the several estimations of Messrs. Thorpe and Morton by $\frac{14·3047}{33·8385} = ·4227$, we obtain the respective amounts of the various constituents which would be present, in a mixture of water from the Irish Sea with distilled water, containing 14·3047 grammes of solid matter per litre.

* *Ann. Chem. Pharm.* clviii. 122–131.

	Water from the Irish Sea so diluted with distilled water as to contain 14·3047 grammes solid matter per litre.	Water from Botallack taken from workings under the sea, "Crown Mine."
	Grammes per litre, calculated.	Grammes per litre, found.
Chlorine . . .	7·8734	6·2778
Iodine	,,	trace
Bromine . . .	·0259	trace
Sulphuric acid .	1·0959	1·2129
Silica	,,	trace
Alumina . . .	,,	·0064
Lime	·2431	1·9970
Magnesia . . .	·8590	·2182
Ferric oxide . .	·0019	·0143
Alkaline chlorides.	11·4905	9·7556
Potassium . . .	·1654	·1392
Lithium . . .	trace	·0119
Sodium . . .	4·3969	3·7054
Ammonia . . .	trace	not estimated
Carbonic acid .	·0096	2·2114
Nitric acid . .	·0006	not estimated

In the above Table a general accordance will be observed between the figures obtained by analysis of the Botallack water and those calculated on an assumed mixture of sea-water with distilled water; it will be remarked, however, that lithium and large quantities of lime and carbonic acid have been taken up. A comparison of the figures in the two columns also renders it evident that magnesia has been in some way abstracted from solution, although the rocks in which the lode is enclosed have been found to contain much larger quantities of magnesia than of lime[*].

Copper, zinc, lead, tin, &c. were carefully sought for by the usual tests without success; but the spectroscope was only employed for examining the alkaline chlorides with the flame of an ordinary Bunsen gas-burner. It is, however, probable that traces of various substances, present in such minute quantities as to escape detection by ordinary means, might be found by spectroscopic observation with the assistance of a powerful induction-coil; and I therefore propose to make further investigations by the aid of this instrument, and also to operate on very large quantities of the various waters subjected to analysis.

* Analyses of two of the clay-slates from this locality afforded respectively:—lime 4·05, magnesia 6·58 per cent.; and lime 4·78, magnesia 11·61 per cent. Phil. Mag. February 1871.

A difficulty in all investigations of this class must necessarily arise from the impossibility of determining the distance which the waters, in each case, have passed through the several lodes. It is consequently quite possible that samples of water may have been examined which, after passing for long distances through the enclosing rocks, may have ultimately entered the veins a few feet only from the point at which they were collected.

Should, however, one specimen of water be found to contain appreciable amounts of the constituents of the vein from which it issued, whilst another is comparatively, or entirely, free from them, it might be inferred, all other conditions being the same, that the first had traversed a greater extent of vein-matter than the second.

It is evident that much time must be expended and numerous analyses made before reliable conclusions can be arrived at ; and should waters containing appreciable quantities of the constituents of the veins from which they have issued be discovered, an important question will still remain unanswered,—Is the water ascending through a metalliferous vein ever the medium from which fresh deposits of mineral matter are being produced at the present time ? or does it, on the contrary, take up and carry away some of the constituents of the lode, giving rise to new combinations and a re-arrangement of its elements ? That both these actions may sometimes be going on simultaneously does not appear improbable.

LII. *Origin of Nerve-force.* By James St.-Clair Gray, *M.B.C.M., F.F.P. & S.G., Assistant to the Professor of Medical Jurisprudence, Glasgow University*[*].

IN the ' Chemical News ' of date August 11, 1871, I drew attention to the fact that, by the action of a solution of caustic potash on sulphur and phosphorus, there was developed an electric current of which the electromotive power, as registered by Sir William Thomson's electrometer, was greater than that of a Daniell's cell, the ratio of the power produced being as four is to three.

The object which I had in view in first making the investigations above referred to, was to obtain some proof in support of a theory which a considerable time ago occurred to me relative to the source of the nerve-power.

According to this theory, I assumed in the first place that the nerve-power had in it an electric element, but failed for some time to discover any satisfactory source whence this agency could be derived. After a lengthened contemplation of the various

constituent elements of the body, it occurred to me that sulphur and phosphorus might be those to which I should look. Acting, then, from the fact that in the brain there was a very considerable proportion of phosphorus, that in the liver there was present a large proportion of sulphur, while between the two there was in constant circulation an alkaline fluid, the blood acting on these facts, and having in my mind the idea that nerve-power and an electrical current, if not identical, were closely related to each other, I constructed the cell already mentioned, with the above result. Having, then, by this experiment determined that an electric current was produced in the cell containing the sulphur and phosphorus in alkaline solution, I turned my attention to the actual conditions (the conditions found to exist in the living animal), and by the following experiment proved the existence between the brain and liver of an electric current.

In the first place, the hind leg of a frog was prepared as a galvanoscope, according to the directions first given by Galvani, and which were followed out so carefully and successfully by Aldini and Matteucci; then to a rabbit 21 oz. in weight chloroform was administered till complete anæsthesia was produced. An incision was then made through the abdominal walls in the right hypochondriac region, and through this aperture a properly insulated copper wire was passed into the substance of the liver; the eyeball was then pierced, and a similar piece of copper wire brought into contact with the brain by forcing it through the optic foramen. The free extremities of the copper wires were then brought into contact with the exposed sciatic nerve of the frog's limb, when powerful convulsions were induced in the muscles receiving their nervous supply therefrom.

Having, then, by this experiment proved that between the brain and the liver there exists an electric current, it is, I think, quite feasible to assume that at least a portion, if not the whole, of this current is due to the action of the alkaline medium on the sulphur and phosphorus contained respectively in the liver and brain, which current we have already found to be produced in the sulphur-and-phosphorus cell. That in the animal economy other sources of electricity do exist I should be the last to deny; but that this, as a source of nervo-motor power, is perhaps second to none receives confirmation, I think, from a consideration of the amount of phosphoric acid excreted by the kidneys as phosphate of soda, potash, lime, and ammonia, amounting on an average to rather more than 72 grains per diem, and of sulphuric acid, as sulphate of soda and potash, amounting to nearly 100 grains, the oxidation-products of sulphur and phosphorus being in the main derived from the two organs in which they in the greatest proportion abound.

Taking, then, for granted that we have here arrived, in part at least, at a solution of the source of nervo-motor power, I look upon the sympathetic nerve, its branches and ganglia, not as a separate or isolated system, but merely as a constituent part of the general nervous system—a part, however, to which is assigned the function of guiding and regulating the movements of involuntary muscular fibre, receiving from the common source its nervo-motor power, but moulding it to its own purposes and requirements; while I think it not at all impossible that in the great serous cavities of the body (the peritoneum, pleura, pericardium, and in those of the encephalon) we may find an arrangement to exist in many respects analogous to Leyden jars.

These ideas may appear to many crude and imperfect; but the subject is still being investigated, and in a future Number additional facts shall be communicated.

15 Newton Terrace, Glasgow,
    September 4, 1871.

LIII. *On* Canon Moseley's views *upon Glacier-motion.* By WILLIAM MATHEWS, *President of the Alpine Club*[*].

THE argument by which Canon Moseley attempts to prove that the descent of glaciers by their weight alone is a mechanical impossibility, as contained in his communication to the Royal Society, read January 7, 1869, may be stated in the following propositions :—

1. In every transverse section of a glacier every particle of ice is, at the same moment of time, moving over and alongside its neighbours.

2. The absolute motion of any point in the surface of a glacier is proportional to its distance from the nearest side, and to its height from the bottom of the channel.

3. This differential motion can only take place by the process which, in mechanics, is known by the name of shear.

4. The resistance which ice offers to shearing, or its shearing-force, as ascertained by experiment in the shearing-apparatus devised by Canon Moseley, is not less than 75 lbs. per square inch.

5. But in order that the Mer de Glace may descend by its own weight, at the rate at which Professor Tyndall observed it descending at the Tacul, its shearing-force per square inch cannot be more than 1·3193 lb.

I propose in the present communication to examine these propositions.

The first has been challenged more than once in the course of the controversy, without eliciting any rejoinder from Canon Moseley, no doubt from the absence of any materials available for the support of the hypothesis. The fact is, that while we have numerous observations of the absolute motion of various points of the surfaces of glaciers, observers do not appear to have been sufficiently alive to the importance of attending to the differential motion, of determining the law of its variation from molecule to molecule, and of ascertaining whether it is continuous or not.

Observations of this kind are by no means easy to make, and require to be conducted with great care and delicacy, errors which might safely be disregarded in a determination of average daily velocity becoming serious when relative and not absolute motion is the object of investigation. These errors arise from the difficulty of boring with the augur vertical holes in the ice, of driving the stakes vertically into the holes that have been bored, of renewing the holes in the same vertical when the glacier has melted away from the stakes, and from the constant tendency of the stakes to heel over to the southward in consequence of their heated faces enlarging the holes in the direction of the sun.

During a short tour in the Alps in the autumn of 1870, I attempted, in concert with my friend Mr. A. Adams Reilly, to make some observations upon differential motion, and selected the side of the Great Aletsch Glacier as the field of our operations.

By means of a well-defined station on the right bank of the glacier, and a well-defined object on the left bank, we ranged out a line between 60 and 70 yards long. We drove our first stake into the ice 20 feet from the station, as near to the edge of the glacier as we could conveniently get it. I shall denote this stake by 0. We had intended to stake out the line every ten yards; but, from certain local difficulties, we were obliged to drive in stake 1 at a distance of nine yards from 0. Stake 2 was eleven yards from 1; and the remaining four stakes were placed at successive distances of ten yards each. The line between 0 and 1 was staked out into nine subdivisions of three feet each, and the space between 5 and 6 into five subdivisions of six feet.

Our work was completed in the afternoon of Monday, the 22nd of August; and the line was reexamined on Wednesday, the 24th, after an interval of forty-eight hours.

In the first place, the spaces between the stakes were carefully remeasured, with the following results :—

Number of stake.	August 22. Distances.		August 24. Distances.	
0.	ft.	in.	ft.	in.
1. . . .	27	0	26	10
2. . . .	33	0	33	0
3. . . .	30	0	30	0
4. . . .	30	0	30	4
5. . . .	30	0	30	0
6. . . .	30	0	30	0·5
	180	0	180	2·5

We were surprised to find that during the two days' interval the space between 0 and 1 had been shortened by two inches. It is not probable that this was due to an error of observation, as the difference was found to be distributed over most of the subdivisions. The elongation of the space between 3 and 4 was due to the widening of a crevasse which crossed the line obliquely in that part.

The following are the absolute and relative motions of the stakes during the two days' interval :—

Number of stake.	Absolute motion. inches.	Relative motion. inch.
0. . . .	2·50	
1. . . .	3·00	0·50
2. . . .	4·00	1·00
3. . . .	5·25	1·25
4. . . .	3·50	−1·75
5. . . .	4·50	1·00
6. . . .	5·00	0·50

These figures show an increase of differential motion in proceeding from the edge of the glacier to a point about thirty yards distant, and a subsequent decrease in proceeding towards the centre ; with a relative regression of the ice in the neighbourhood of stake 4, as indicated by the negative sign. The greatest differential motion is between stakes 3 and 4. It amounts to no more than ·875 inch in twenty-four hours over a distance of 360 inches, or about $\frac{1}{400}$ of an inch in twenty-four hours in points one inch apart. Between 5 and 6 it is only $\frac{1}{1440}$ of an inch for the same time and space.

The displacements of the stakes intermediate between 0 and 1, and 5 and 6, were also determined. Each intermediate stake was found to share in the differential motion.

The measurements were confined to a breadth of 60 yards in a part of the glacier where the distance from side to centre was not less than 600 yards, and consequently only exhibit the deportment of the side ice. It was important to supplement them by examining a glacier in the central portion of the stream; and, I

being obliged to return home, Mr. Reilly devoted three weeks to this purpose, and has generously placed his notes at my disposal.

The spot selected for his first operations was a part of the Glacier of Bionassay, where the stream is very slightly inclined, and the central portion nearly level from side to side and free from crevasses. The width of the glacier at this part was 320 yards, for 200 yards of which, measuring from the left bank, the surface was composed of "avalanche-ice without veined structure," the remaining 120 yards being ordinary glacier-ice.

Mr. Reilly bored the first hole about 70 yards from the right bank of the glacier, and ranged, with a theodolite, a line 170 yards in length, terminating about 80 yards from the left bank. This line was divided into seventeen equal spaces by holes bored 10 yards apart; the line was ranged and staked on the 7th of September. The holes were deepened from time to time as the glacier-surface melted, and the final measurements made on the 27th, after an interval of twenty days.

The results are exhibited in the following Table—the motion on each side of No. 10, where the velocity was greatest, being exhibited in parallel columns, the negative signs indicating relative regressions of the ice at the points to which they refer :—

Right moiety.			Left moiety.		
Stations.	Absolute motion.	Relative motion.	Stations.	Absolute motion.	Relative motion.
	ft. in.	in.		ft. in.	in.
0.	11 6·50				
1.	10 11·60	−6·90			
2.	11 4·25	4·65			
3.	11 3·00	−1·25	17.	11 0·50	
4.	11 7·25	4·25	16.	11 3·00	2·50
5.	11 6·75	−0·50	15.	11 7·00	4·00
6.	11 5·25	−1·50	14.	11 6·50	−0·50
7.	11 6·50	1·25	13.	11 8·50	2·00
8.	11 6·00	−0·50	12.	11 7·50	−1·00
9.	11 9·00	3·00	11.	11 7·00	−0·50
10.	11 10·00	1·00	10.	11 10·00	3·00

Here we have a superficial area of ice 170 yards in width moving through a space of nearly 12 feet in twenty days, with an advance of the centre during that interval only 10 inches in excess of the sides, the differential motion at the side being 7 inches, and at the centre 1 inch in a width of 360 inches; so that two points an inch apart would in twenty-four hours move past each other to the extent of a little less than the $\frac{1}{1000}$ of an inch at the sides, and the $\frac{1}{7000}$ of an inch at the centre of the area under consideration.

Mr. Reilly has not supplied me with any note of the motion of the edge of the glacier during the interval; but as the edge of the Great Aletsch was found to move at the rate of only an inch and a quarter in twenty-four hours, it probably did not exceed 2 feet. We shall therefore be justified in saying that while, in the right-hand moiety of the glacier, a differential motion of 10 inches is distributed over a width of 100 yards from the line of maximum velocity, a differential motion of at least 100 inches must be distributed over the remaining 70 yards up to the edge of the glacier.

Two of the 30-feet spaces were staked out into subdivisions of 2 feet each. Each of the intermediate stakes exhibited a differential motion, with occasional negative signs—the greatest relative displacement observed being 2·25 inches in the twenty days, equivalent to the $\frac{1}{213}$ of an inch in twenty-four hours for points 1 inch apart.

During the intervals of his labours on the Glacier of Bionassay Mr. Reilly ranged a line across the Mer de Glace, on the Chamonix side of the Montanvert. His measurements on this line during a period of nineteen days indicate a motion very similar in its character to that of the Glacier of Bionassay. The length of this line from the left-hand edge of the glacier to the point of maximum velocity was about 1000 feet. The central 500 feet had an absolute motion of 18 feet 7·75 inches at its left-hand extremity, and of 21 feet 2 inches at its right, equivalent to a mean daily motion of about 12 inches. Its total differential motion was 28·25 inches, equivalent to a mean daily differential motion of about 1·5 inch, or $\frac{1}{4000}$ of an inch for points 1 inch apart. The nearest station to the edge of the glacier was about 165 feet distant from it; and at this station the absolute motion in nineteen days was found to be 10 feet 11·25 inches. This would indicate a marginal motion of about 4 feet, and would leave us 14 feet 6 inches of differential motion to distribute over the lateral 500 feet of the line, or six times as much as that of the central moiety.

The law of variation of the differential motion indicated by the observations above described is not new. It appears clearly from the measurements made by Professor Tyndall on the Mer de Glace, described in a communication to the Royal Society, read May 20, 1858, and published in vol. cxlix. of the Philosophical Transactions. But nowhere is it brought out with more striking prominence than in the observations made by Agassiz upon the Unter-Aar Glacier, from 1842 to 1845, as described in Chapter XII. of the *Nouvelles Études,* and in plate 4 of the accompanying atlas, where curves showing the motion, for three consecutive years, of a series of points originally in a straight

line, are plotted to scale. The diagram exhibits the very small motion of points close to the side, whence the curves extend with *their concavity downwards* as far as the point of maximum differential velocity, where they become convex, and gradually increase in curvature up to about one fourth of the width of the glacier, whence they sweep across to the corresponding point on the opposite side in a curve so flattened as to be scarcely distinguishable from a straight line.

The above considerations lead to the following conclusions upon the five fundamental propositions of Canon Moseley.

1. It is probable that every molecule of a glacier moves with a very slow differential motion, which, whenever the ice is continuous, is continuous from molecule to molecule, and from moment to moment of time.

2. The hypothesis that the differential motion is uniform from centre to side is wholly contrary to fact. The semisurface of every glacier may be roughly divided into two equal longitudinal strips, through the lateral of which from 80 to 90 per cent. of the differential motion is distributed, while the central strip moves downwards almost like a rigid body, with a large reserve of gravitating force capable of affecting the sides.

Canon Moseley is of opinion that this divergence between theory and fact greatly strengthens his position ; but he has not made good this part of his case.

3, 4. The Canon has failed, as it seems to me, to establish any analogy between the disruption of adjacent surfaces of a solid body in a shearing-machine and the slow relative displacements of the molecules of a glacier. He has yet to prove that, because he was obliged to employ a force of 75 lbs. per square inch to shear asunder adjacent surfaces of a solid cylinder of ice through a space of 1 inch in half an hour, it would require as great a force to produce the relative displacements which occur in an actual glacier,—the latter having been shown, from the observations above described, to range for molecules the tenth of an inch apart, and an interval of twenty-four hours, from the $\frac{1}{2130}$ to the $\frac{1}{70,000}$ of an inch[*].

5. On the other hand, the slow continuous displacements of the molecules of a glacier are *undistinguishable in kind* from the displacements of the molecules of an ice-plank, supported at its extremities and allowed to subside under the influence of its weight, displacements which require for their production, as I have shown in the Philosophical Magazine for November 1871, a force considerably less than $1\frac{1}{3}$ lb. per square inch—less, therefore, than the very force which the Canon considers sufficient to shear the Mer de Glace if it descends by its own gravitation.

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[*] This objection has been forcibly urged by Mr. Ball in the Philosophical Magazine for July 1870.

LIV. *On a Class of Definite Integrals.*—Part II. *By* J. W. L. GLAISHER, *B.A., F.R.A.S., Fellow of Trinity College, Cambridge**.

BEFORE noticing the applications and Tables of the Error-function referred to in my previous communication on the subject, it seems desirable to supplement the integrals already obtained by several additional formulæ.

The integral $\int_0^x e^{-u^2} du$ is so frequently used that it is convenient to have a separate notation for it apart from its value $\frac{1}{2}\sqrt{\pi} - \operatorname{Erf} x$. Denoting this integral, therefore, by Erfc x (*i. e.* the *Error-function-complement* of x)†, we have

$$\int_x^\infty e^{-u^2} du = \operatorname{Erf} x, \quad \int_0^x e^{-u^2} du = \operatorname{Erfc} x,$$

and

$$\operatorname{Erf} x + \operatorname{Erfc} x = \frac{\sqrt{\pi}}{2}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (26)$$

x being supposed positive. If x be negative, since

$$\operatorname{Erf}(-x) = -\operatorname{Erf} x,$$

the relation is

$$\operatorname{Erf} x + \operatorname{Erfc} x = -\frac{\sqrt{\pi}}{2}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (27)$$

This enables us to express in a simple manner some of the integrals previously obtained; for instance, (12) may be written

$$\int_0^\infty e^{-a^2 x^2} \sin 2rx \, \frac{dx}{x} = \sqrt{\pi} \operatorname{Erfc}\left(\frac{r}{a}\right).$$

In his *Exercices de Mathématiques*, 1827, Cauchy has given the theorem

$$e^{-x^2} + e^{-(x-a)^2} + e^{-(x+a)^2} + e^{-(x-2a)^2} + e^{-(x+2a)^2} + \dots$$
$$= \frac{2\sqrt{\pi}}{a}\left\{\frac{1}{2} + e^{-\frac{\pi^2}{a^2}}\cos\frac{2\pi x}{a} + e^{-\frac{4\pi^2}{a^2}}\cos\frac{4\pi x}{a} + \dots\right\}; \quad (28)$$

from which, by writing $2a$ for a, and subtracting the original

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* Communicated by the Author.

† This notation is in harmony with that adopted in the case of the sine and cosine; the cosine of x is the sine of the complement of x (not the complement of $\sin x$), while Erfc x (in which the letter standing for complement is at the end of the word) denotes the complement of Erf x. Similarly in the case of the sine-integral, it would be convenient to write Sie x for $\frac{1}{2}\pi - \operatorname{Si} x$.

series from the double of the series so formed, we obtain[*]

$$e^{-x^2} - e^{-(x-a)^2} - e^{-(x+a)^2} + e^{-(x-2a)^2} + e^{-(x+2a)^2} - \ldots$$
$$= \frac{2\sqrt{\pi}}{a} \left\{ e^{-\frac{\pi^2}{4a^2}} \cos\frac{\pi x}{a} + e^{-\frac{9\pi}{4a^2}} \cos\frac{3\pi x}{a} + \therefore \right\}. \quad (29)$$

Integrating these series between the limits x and 0, we find

$$\mathrm{Erfc}\, x + \mathrm{Erfc}\, (x-a) + \mathrm{Erfc}\, (x+a) + \ldots$$
$$= \frac{x\sqrt{\pi}}{a} + \frac{1}{\sqrt{\pi}} \left\{ e^{-\frac{\pi^2}{a^2}} \sin\frac{2\pi x}{a} + \frac{1}{2} e^{-\frac{4\pi^2}{a^2}} \sin\frac{4\pi x}{a} + \ldots \right\}, \quad (30)$$

$$\mathrm{Erfc}\, x - \mathrm{Erfc}\, (x-a) - \mathrm{Erfc}\, (x+a) + \ldots$$
$$= \frac{2}{\sqrt{\pi}} \left\{ e^{-\frac{\pi^2}{4a^2}} \sin\frac{\pi x}{a} + \frac{1}{3} e^{-\frac{9\pi^2}{a^2}} \sin\frac{3\pi x}{a} + \ldots \right\}. \quad (31)$$

These formulæ, however, involve an ambiguity; for on the left-hand side the terms at a great distance from the commencement of the series take the form $\frac{\sqrt{\pi}}{2} (1-1+1- \ldots)$; and if we substitute $\frac{\sqrt{\pi}}{2} - \mathrm{Erf}\, x$ for $\mathrm{Erfc}\, x$ &c., we still introduce the same indeterminate series. The difficulty would not be avoided by integrating between the limits ∞ and x instead of x and 0; for then, on the right-hand side, we should introduce $\sin \infty$ into both formulæ, and in addition an infinite term in (30).

We should, however, from the results of similar inquiries, be inclined to suspect that in point of fact we must, with the exception of the first term, take the terms in pairs, so as not to end with a term $\mathrm{Erfc}\, (x \pm na)$ without including also $\mathrm{Erfc}\, (x \mp na)$ (n infinite); and the following independent investigation will show that this is the case.

From the integral (21) of the previous paper we find that

$$\int_0^\infty \mathrm{Erf}\, x \sin\frac{2n\pi x}{a}\, dx = \frac{a}{4n\sqrt{\pi}} (1 - e^{-\frac{n^2\pi^2}{a^2}}),$$

whence

$$\int_0^a \{ \mathrm{Erf}\, x + \mathrm{Erf}\, (x-a) + \mathrm{Erf}\, (x+a) + \ldots \} \sin\frac{2n\pi x}{a}\, dx$$
$$= \frac{a}{2n\sqrt{\pi}} (1 - e^{-\frac{n^2\pi^2}{n^2}});$$

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[*] The series (29) is given by Sir W. Thomson (Quarterly Journal of Mathematics, vol. i. p. 316); and (31) is deduced by integration; the ambiguity, however, is not noticed.

and therefore, by Fourier's theorem, between the limits 0 and a of x,

$$\mathrm{Erf}\, x + \mathrm{Erf}\,(x-a) + \mathrm{Erf}\,(x+a) + \ldots$$

$$= \frac{1}{\sqrt{\pi}} \left\{ \sin \frac{2\pi x}{a} + \frac{1}{2} \sin \frac{4\pi x}{a} + \ldots - e^{-\frac{\pi^2}{a^2}} \sin \frac{2\pi x}{a} \right.$$
$$\left. - \frac{1}{2} e^{-\frac{4\pi^2}{a^2}} \sin \frac{4\pi x}{a} - \ldots \right\}$$

$$= \frac{1}{2} \sqrt{\pi} - \frac{x\sqrt{\pi}}{a} - \frac{1}{\sqrt{\pi}} \left\{ e^{-\frac{\pi^2}{a^2}} \sin \frac{2\pi x}{a} + \frac{1}{2} e^{-\frac{4\pi^2}{a^2}} \sin \frac{4\pi x}{a} + \ldots \right\} \quad (32)$$

on replacing the former trigonometrical series by its sum $\frac{1}{2}\left(\pi - \frac{2\pi x}{a} \right)$.

We can either deduce from this equation or prove independently that

$$\mathrm{Erf}\, x - \mathrm{Erf}\,(x-a) - \mathrm{Erf}\,(x+a) + \ldots$$
$$= \frac{1}{2}\sqrt{\pi} - \frac{2}{\sqrt{\pi}} \left\{ e^{-\frac{\pi^2}{4a^2}} \sin \frac{\pi x}{a} + \frac{1}{3} e^{-\frac{9\pi^2}{a^2}} \sin \frac{3\pi x}{a} + \ldots \right\}. \quad (33)$$

If x be negative in (32) or (33), the sign of the constant $\frac{1}{2}\sqrt{\pi}$ must, of course, be changed.

Putting $x = \frac{1}{2}a$ in (33), we find

$$\mathrm{Erf}\, a - \mathrm{Erf}\, 3a + \mathrm{Erf}\, 5a - \ldots$$
$$= \frac{1}{4}\sqrt{\pi} - \frac{1}{\sqrt{\pi}} \left(e^{-\frac{\pi^2}{16a^2}} - \frac{1}{3} e^{-\frac{9\pi^2}{16a^2}} + \ldots \right), \quad (34)$$

writing a for $\frac{1}{2}a$.

This formula, (34), I have verified numerically to seven decimal places when $a = \frac{1}{2}$.

In (29) put $x = 0$, and we have

$$e^{-a^2} - e^{-4a^2} + e^{-9a^2} - \ldots = \frac{1}{2} - \frac{\sqrt{\pi}}{a} \left\{ e^{-\frac{\pi^2}{4a^2}} + e^{-\frac{9\pi^2}{a^2}} + \ldots \right\}. \quad (35)$$

Integrate this between the limits x and 0, and we obtain an interesting formula connecting the error-function and the exponential-integral, viz.

$$\mathrm{Erfc}\, x - \frac{1}{2} \mathrm{Erfc}\, 2x + \frac{1}{3}\mathrm{Erfc}\, 3x - \ldots$$
$$= \frac{1}{2}x + \frac{\sqrt{\pi}}{2} \left\{ \mathrm{Ei}\left(-\frac{\pi^2}{4x^2} \right) + \mathrm{Ei}\left(-\frac{9\pi^2}{4x^2} \right) + \ldots \right\}, \quad (36)$$

since

$$\int_0^x \frac{e^{-\frac{n^2}{a^2}}}{a}\, da = \int_{\frac{1}{x}}^{\infty} e^{-n^2 a^2} \frac{da}{a} = \frac{1}{2} \int_{\frac{1}{x^2}}^{\infty} e^{-n^2 a} \frac{da}{a} = -\frac{1}{2}\mathrm{Ei}\left(-\frac{n^2}{x^2} \right)$$

Putting $x=0$ in (28), we find

$$\frac{1}{2}+e^{-a^2}+e^{-4a^2}+\ldots=\frac{\sqrt{\pi}}{a}\left\{\frac{1}{2}+e^{-\frac{\pi^2}{a^2}}+e^{-\frac{4\pi^2}{a^2}}+\ldots\right\}, \quad (37)$$

which on integration between limits gives

$$\frac{a-b}{2}+(\operatorname{Erf} b-\operatorname{Erf} a)+\frac{1}{2}(\operatorname{Erf} 2b-\operatorname{Erf} 2a)+\ldots=\frac{\sqrt{\pi}}{2}\log\frac{a}{b}$$
$$+\frac{\sqrt{\pi}}{2}\left\{\operatorname{Ei}\left(-\frac{\pi^2}{b^2}\right)-\operatorname{Ei}\left(-\frac{\pi^2}{a^2}\right)+\operatorname{Ei}\left(-\frac{4\pi^2}{b^2}\right)-\operatorname{Ei}\left(-\frac{4\pi^2}{a^2}\right)+\ldots\right\};$$

and many similar formulæ could, no doubt, be obtained.

It is a matter of some importance in regard to the use of the function to be enabled to replace it, when the argument is imaginary, by a real integral; this may, of course, be done in many ways, but probably the most convenient forms will be found to be those deduced from the integral

$$\int_0^\infty e^{-ax^2-2bx}\,dx=\frac{1}{\sqrt{a}}\,e^{\frac{b^2}{a}}\,\operatorname{Erf}\frac{b}{\sqrt{a}}.$$

Writing in this integral $a(\cos\alpha+i\sin\alpha)$ and $b(\cos\beta+i\sin\beta)$ for a and b respectively, there results

$$\int_0^\infty e^{-ax^2\cos\alpha-2bx\cos\beta}\{\cos(ax^2\sin\alpha+2bx\sin\beta)$$
$$-i\sin(ax^2\sin\alpha+2bx\sin\beta\}\,dx$$
$$=\frac{1}{\sqrt{a}}\,e^{\frac{b^2}{a}\cos(2\beta-\alpha)}\left[\cos\left\{\frac{b^2}{a}\sin(2\beta-\alpha)-\frac{1}{2}\alpha\right\}\right.$$
$$+i\sin\left\{\frac{b^2}{a}\sin(2\beta-\alpha)-\frac{1}{2}\alpha\right\}\left.\right]\operatorname{Erf}\frac{b}{\sqrt{a}}\left\{\cos\left(\beta-\frac{1}{2}\alpha\right)\right.$$
$$\left.+i\sin\left(\beta-\frac{1}{2}\alpha\right)\right\}. \quad (38)$$

Numerous special forms are deducible for $\operatorname{Erf}(\mathrm{A}+\mathrm{B}i)$ by giving particular or zero values to a, b, α, and β; $a\cos\alpha$, however, must not be made negative.

Two of the most simple forms are given at the end of the previous paper; from them we see that, to form a Table of the error-function for arguments of the form $a+bi$, it would be necessary to tabulate

$$e^{-a^2}\int_0^b e^{x^2}\sin 2ax\,dx \quad\text{and}\quad e^{-a^2}\int_0^b e^{x^2}\cos 2ax\,dx,$$

or

$$e^{b^2}\int_a^\infty e^{-x^2}\sin 2bx\,dx \quad\text{and}\quad e^{b^2}\int_a^\infty e^{-x^2}\cos 2bx\,dx.$$

The Table would be of double entry, and its calculation would entail more labour than the importance of the function at present merits. By the comparison of the two forms for

$$\text{Erf}\,(a+bi)\pm\text{Erf}\,(a-bi)$$

just referred to, we have incidentally* (putting $c=1$) the two theorems

$$e^{b^2}\int_a^\infty e^{-x^2}\sin 2bx\,dx = e^{-a^2}\int_0^b e^{x^2}\cos 2ax\,dx, \quad . \quad . \quad (39)$$

$$e^{b^2}\int_a^\infty e^{-x^2}\cos 2bx\,dx + e^{-a^2}\int_0^b e^{x^2}\sin 2ax\,dx = \text{Erf}\,a. \quad . \quad (40)$$

Denoting for brevity $\dfrac{1}{\sqrt{a}}e^{\frac{b^2}{4a}}\text{Erf}\dfrac{b}{2\sqrt{a}}$ by u, we see from (10)

that $\left(-\dfrac{d}{da}\right)^n u = \left(\dfrac{d}{db}\right)^{2n} u,$ which on taking $2\sqrt{a}=\alpha$, and replacing

α by a, assumes the form

$$\left(\frac{d}{db}\right)^{2n}\frac{b^2}{e^{a^2}}\text{Erf}\frac{b}{a} = a\left(-\frac{2d}{ada}\right)^n\frac{1}{a}e^{\frac{b^2}{a^2}}\text{Erf}\frac{b}{a},$$

or, as we may write it after an obvious transformation,

$$\left(\frac{d}{db}\right)^{2n}e^{a^2b^2}\text{Erf}\,ab = \frac{1}{a}\left(2a^3\frac{d}{da}\right)^n a e^{a^2b^2}\text{Erf}\,ab, \quad . \quad . \quad (41)$$

a good instance of a result obtained at once from a definite integral, but which it would not be easy to prove otherwise.

Among miscellaneous formulæ may be noticed

$$\int_0^\infty e^{-\frac{a^2}{x^2}}\text{Erf}\,(cx)\frac{dx}{x} = -\frac{\sqrt{\pi}}{2}\text{Ei}\,(-2ac) = \int_0^\infty e^{-a^2x^2}\text{Erf}\left(\frac{c}{x}\right)\frac{dx}{x}\ (42)$$

$$\int_0^\infty(\text{Erf}\,ax-\text{Erf}\,bx)\cos 2rx\,\frac{dx}{x} = \frac{\sqrt{\pi}}{4}\left\{\text{Ei}\left(-\frac{r^2}{a^2}\right)-\text{Ei}\left(-\frac{r^2}{b^2}\right)\right\}\ (43)$$

$$\int_0^\infty\text{Erf}\sqrt{c(x^2+a^2)}\frac{dx}{\sqrt{(x^2+a^2)}} = -\frac{\sqrt{\pi}}{4}\text{Ei}\,(-a^2c), \quad . \quad . \quad (44)$$

deduced by integration from the formula intermediate to (19) and (20), from (11), and from a formula intermediate to (4) and (5) respectively.

From No. 17, Table 267, and No. 1, Table 266 of De Haan's *Nouvelles Tables d'Intégrales définies,* we deduce by dividing by p (p^2 having been previously written for p in the latter integral), and integrating with respect to p between the limits ∞ and q,

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* On page 301, bottom line but one, e^{-2a^2c} should be e^{-a^2c}.

that

$$\int_0^\infty \frac{\sin (2a+1)x}{\sin x} \operatorname{Ei}(-q^2x^2)dx$$
$$= -2\sqrt{\pi}\left\{\frac{1}{2q} + \operatorname{Erfc}\frac{1}{q} + \frac{1}{2}\operatorname{Erfc}\frac{2}{q} \ldots + \frac{1}{a}\operatorname{Erfc}\frac{a}{q}\right\}, \quad (45)$$

and

$$\int_0^\infty \frac{\operatorname{Ei}(-q^2x^2)\,dx}{1-2r\cos x+r^2}$$
$$= -\frac{4}{1-r^2}\left\{\frac{1}{8q} + r\operatorname{Erfc}\frac{1}{2q} + \frac{r^2}{2}\operatorname{Erfc}\frac{2}{2q} + \ldots\right\}, \quad (46)$$

the latter series extending to infinity.

A method similar to one frequently given for the evaluation of $\int_0^\infty e^{-x^2}\,dx$ enables us to express the product of two error-functions as a single integral for

$$\operatorname{Erfc} a \operatorname{Erfc} b = \int_0^a\int_0^b e^{-x^2-y^2}\,dx\,dy,$$

which on transforming to polar coordinates becomes

$$= \frac{1}{2}\left\{\int_0^{\tan^{-1}\frac{b}{a}}(1-e^{-a^2\sec^2\theta})d\theta + \int_{\tan^{-1}\frac{b}{a}}^{\frac{1}{2}\pi}(1-e^{-b^2\operatorname{cosec}^2\theta})\,d\theta\right\}$$

$$= \frac{\pi}{4} - \frac{1}{2}ae^{-a^2}\int_0^b \frac{e^{-x^2}\,dx}{a^2+x^2} - \frac{1}{2}be^{-b^2}\int_0^a \frac{e^{-x^2}\,dx}{b^2+x^2} \quad \cdot \quad \cdot \quad \cdot \quad (47)$$

after a couple of obvious transformations.

Taking $a = b$, we find

$$(\operatorname{Erfc} a)^2 = \frac{\pi}{4} - e^{-a^2}\int_0^1 \frac{e^{-a^2x^2}\,dx}{1+x^2},$$

whence

$$\int_0^1 \frac{e^{-a^2x^2}\,dx}{1+x^2} = e^{a^2}\{\sqrt{\pi}\operatorname{Erf} a - 2(\operatorname{Erf} a)^2\}. \quad \cdot \quad \cdot \quad (48)$$

The equation (48) is not new, being only a simple transformation of one given by Raabe in 1847[*]. Raabe shows that

$$\int_0^\infty \frac{e^{-ax^2}}{1+x^2}\,dx = e^{\frac{1}{2}a}\sqrt{\pi}\left\{\frac{1}{2}\sqrt{\pi}e^{\frac{1}{2}a} - \sqrt{f(a)}\right\},$$

[*] "Ueber Producte und Potenzen bestimmter einfacher Integral-Ausdrücke, durch mehrfache dargestellt," Crelle's *Journal,* vol. xlviii. p. 137. See also De Haan's *Nouvelles Tables,* No. 2. T. 29, and No. 5. T. 80.

where

$$f(a) = a + \left(1 - \frac{1}{3}\right)\frac{a^2}{1 \cdot 2} + \left(1 - \frac{1}{3} + \frac{1}{5}\right)\frac{a^3}{1 \cdot 2 \cdot 3} + \cdots.$$

Comparing this result with (5), we find

$$\mathrm{Erfc}\, x = e^{-\frac{1}{2}x^2}\sqrt{f(x^2)}. \quad \ldots \quad (49)$$

The forms assumed by (5) and (44) when x is taken $= \tan\theta$ are worthy of attention from their simplicity; we have

$$\int_0^{\frac{1}{2}\pi} e^{-a\tan^2\theta}\, d\theta = \sqrt{\pi}\, e^a\, \mathrm{Erf}\, \sqrt{a}, \quad \ldots \quad (50)$$

and

$$\int_0^{\frac{1}{2}\pi} \mathrm{Erf}\,(a\sec\theta)\frac{d\theta}{\cos\theta} = -\frac{\sqrt{\pi}}{4}\,\mathrm{Ei}\,(-a^2). \quad (51)$$

Writing x^2 for x in (18), we find the area of the curve $y = \mathrm{Erf}\, ax$, viz.

$$\int_0^\infty \mathrm{Erf}\,(ax)\, dx = \frac{1}{2a}. \quad \ldots \quad (52)$$

For the numerical calculation of the values of the error-function, there are three series, viz.

$$\mathrm{Erfc}\, x = x - \frac{x^3}{3} + \frac{1}{1 \cdot 2}\frac{x^5}{5} - \frac{1}{1 \cdot 2 \cdot 3}\frac{x^7}{7} + \cdots, \quad \ldots \quad (53)$$

$$\mathrm{Erfc}\, x = xe^{-x^2}\left(1 + \frac{2x^2}{1 \cdot 3} + \frac{(2x^2)^2}{1 \cdot 3 \cdot 5} + \frac{(2x^2)^3}{1 \cdot 3 \cdot 5 \cdot 7} + \cdots\right), \quad (54)$$

$$\mathrm{Erf}\, x = \frac{e^{-x^2}}{2x}\left(1 - \frac{1}{2x^2} + \frac{1 \cdot 3}{(2x^2)^2} - \frac{1 \cdot 3 \cdot 5}{(2x^2)^3} + \cdots\right), \quad \ldots \quad (55)$$

and the continued fraction

$$\mathrm{Erf}\, x = \frac{e^{-x^2}}{2x}\frac{1}{1+}\frac{1}{2x^2+}\frac{2}{1+}\frac{3}{2x^2+}\frac{4}{1+\&\mathrm{c}.} \quad \ldots \quad (56)$$

The formula (34) might also be of use in the calculation of the integral.

The discontinuity in the values of the constants and other points connected with the series (55), when x is of the form $a + bi$, are fully discussed by Professor Stokes in a memoir "On the Discontinuity of Arbitrary Constants which appear in Divergent Developments," Camb. Phil. Trans. vol. x.

Applications of the Error-function to Physics &c.

The work which first gave the error-function an importance of the first rank in physics was Kramp's *Analyse des Réfractions Astronomiques et Terrestres*, Strasbourg, 1798. Kramp shows

(page 36) that "all the great problems of astronomical and terrestrial refraction depend on the integration of the differential equation"

$$dR = - \frac{\omega Y v \, dv}{c \sqrt{(1 - v^2 - 2\omega + 2\omega Y)}}, \quad \cdot \quad \cdot \quad \cdot \quad (57)$$

in which dR denotes an element of the curvature of the path of the ray (viz. $\frac{ds}{\rho}$, in the usual notation, ρ being the radius of curvature), $v = \frac{a \sin A}{y}$ (a being the earth's radius, A the angle of incidence of the ray at the surface of the earth, and y the radius vector from the centre of the earth to a point of the ray's path), Y the density of the air $= e^{\frac{v - \sin A}{c}}$, while c and ω (which are very small) are independent of Y, and also therefore of v.

The limits of integration are a and ∞ for y, and therefore $\sin A$ and 0 for v.

To effect the integration, Kramp expands the right-hand side of (57) in a series proceeding according to ascending powers of ω, viz.

$$dR = - \frac{\omega Y v dv}{c(1-v^2)^{\frac{1}{2}}} - \frac{\omega^2 Y(1-Y)vdv}{c(1-v^2)^{\frac{3}{2}}} - \frac{3}{2} \cdot \frac{\omega^3 Y(1-Y)^2 vdv}{c(1-v^2)^{\frac{5}{2}}} - \cdots,$$

and treats each term separately.

To integrate $\dfrac{Y v dv}{(1-v^2)^{n+\frac{1}{2}}}$, put $ct = 1 - v$, and we obtain

$$e^{\frac{K}{c}} c^{-n+\frac{1}{2}} \int_{\frac{K}{c}}^{\infty} (2t - ct^2)^{-n-\frac{1}{2}} (1-ct) e^{-t} dt,$$

K being written for $1 - \sin A$ (Kramp, p. 118).

In the problem of refraction we may neglect powers of c and retain only the first term of the integral, so that we are only concerned with the integral

$$\int_{\frac{K}{c}}^{\infty} t^{-n-\frac{1}{2}} e^{-t} dt ;$$

that is, with

$$\int_{x}^{\infty} t^{-2n} e^{-t^2} dt, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (58)$$

x being written for $\sqrt{\left(\dfrac{1-\sin A}{c}\right)}$. By integration by parts, it is easily shown that (58) is reduced to dependence on the error-function according to the formula

$$(-)^n \int_x^\infty t^{-2n} e^{-t^2} dt = \frac{2^n}{1 \cdot 3 \ldots (2n-1)} \operatorname{Erf} x$$

$$- \frac{2^{n-1}}{1 \cdot 3 \ldots (2n-1)} \left\{ \frac{1}{x} - \frac{1}{2x^3} + \frac{1 \cdot 3}{4x^5} - \cdots \pm \frac{1 \cdot 3 \ldots (2n-3)}{2^{n-1} \, x^{2n-1}} \right\}^*,$$

so that for the calculation of refraction a Table of the values of $\operatorname{Erf} x$ was necessary.

In 1805 the fourth volume of Laplace's *Mécanique Céleste* was published. Chapters I. and II. of Livre X. are devoted to Astronomical and Terrestrial Refraction; and the error-function necessarily occupies a conspicuous place in the investigation. On page 285 † Laplace investigates the continued fraction (56).

To give in a short space any account of the applications of the function to the Theory of Probabilities would be impossible; but the results are so well known that a detailed description of them would be superfluous. In the Theory of Errors of Observations the function is of paramount importance; and this fact is the justification of the name and symbol by which it has been here denoted. Laplace's great work on the subject, the *Théorie analytique des Probabilités,* was published in 1812; but most of the results had previously appeared in various memoirs. The law of facility to which Laplace's, Poisson's, Gauss's, in fact all investigations lead, is represented by e^{-cx^2}, so that the probability of a single error of observation lying between x and $x+dx$ is $\left(\dfrac{c}{\pi}\right)^{\frac{1}{2}} e^{-cx^2} dx$, and the chance of an error lying between p and q is

$$\left(\frac{c}{\pi}\right)^{\frac{1}{2}} \int_p^q e^{-cx^2} dx = \pi^{-\frac{1}{2}} (\operatorname{Erf} p \sqrt{c} - \operatorname{Erf} q \sqrt{c}).$$

It is unnecessary to notice the papers in which the theory of errors and the method of least squares are discussed. De Morgan's treatise in the *Encyclopædia Metropolitana* contains an analysis of Laplace's work; and Todhunter's 'History of Probabilities' supplies a commentary to it. References will be found in a memoir by Ellis, Camb. Phil. Trans. vol. viii. p. 204.

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* Kramp, p. 133.
† The reference is to the National Edition, 1845.

Todhunter remarks (p. 486 of his 'History') that in a memoir of 1783 Laplace pointed out the use of tabulating the integral $\int e^{-t^2} dt$ for different limits.

In the Theory of the Conduction of Heat the function occupies a place of great importance. Thus Fourier proves[*] that if one extremity of a bar be kept at a constant temperature equal to unity, the initial temperature at all points in the rest of the bar being zero, then the temperature v at time t will be given by the formula

$$v = e^{-cx} - \frac{e^{-cx}}{\sqrt{\pi}} \operatorname{Erf}\left(\sqrt{ht} - \frac{x}{2\sqrt{kt}}\right) + \frac{e^{cx}}{\sqrt{\pi}} \operatorname{Erf}\left(\sqrt{ht} + \frac{x}{2\sqrt{kt}}\right),$$

c being written for $\sqrt{\dfrac{h}{k}}.$

On page 514 of the memoir of Fourier's just referred to is given the proposition from which Sir W. Thomson has deduced his results with regard to the secular cooling of the earth[†]. Fourier's problem is, that if in a solid extending to infinity in all directions the temperature has initially two different constant values, viz. unity and zero on the two sides of a certain infinite plane, then at time t the temperature v will be given by the equation

$$v = \frac{2}{\sqrt{\pi}} \operatorname{Erf} \frac{x}{2\sqrt{kt}}.$$

Problems dependent for their solution on the error-function are also discussed by Fourier, *loc. cit.* p. 516 *et seqq.*, and by Riemann, pp. 124, 164, &c. of his *Partielle Differentialgleichungen*, &c. (Braunschweig, 1869). On page 169 Riemann proves[‡] that

$$\int_a^b e^{-x^2 - \frac{c^2}{x^2}} dx = \tfrac{1}{2} e^{2c} \left\{ \operatorname{Erf}\left(a + \frac{c}{a}\right) - \operatorname{Erf}\left(b + \frac{c}{b}\right) \right\}$$

$$+ \tfrac{1}{2} e^{-2c} \left\{ \operatorname{Erf}\left(a - \frac{c}{a}\right) - \operatorname{Erf}\left(b - \frac{c}{b}\right) \right\}. \quad . \quad . \quad (59)$$

A particular case of this elegant theorem was discovered by Boole[§] in 1849; his result may be written

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[*] *Mémoires de l'Institut,* t. iv. (1819 and 1820), p. 508.

[†] Thomson and Tait's 'Natural Philosophy,' vol. i. p. 717, where other references are given.

[‡] Riemann has left out the factor $\tfrac{1}{2}$ on the right-hand side.

[§] Cambridge and Dublin Mathematical Journal, vol. iv. p. 18.

$$\int_0^{\sqrt{c}} e^{-x^2 - \frac{c^2}{x^2}} dx = \frac{\sqrt{\pi}}{4} e^{-2c} - \tfrac{1}{2} e^{2c} \operatorname{Erf}(2\sqrt{c}),$$

agreeing with (59).

It need scarcely be remarked that the function owes its importance in the Theory of Heat to the fact of $u = \operatorname{Erf} \dfrac{x}{2\sqrt{kt}}$ being an integral of

$$\frac{du}{dt} = k \frac{d^2u}{dx^2}.$$

From its uses in physics, it will be evident that the error-function may fairly claim at present to rank in importance next to the trigonometrical and logarithmic functions.

Tables of the Error-function.

For large values of x the formula (55) converges rapidly for some distance, and affords a means of calculating $\operatorname{Erf} x$ and $e^{x^2} \operatorname{Erf} x$ with ease; and for small values the formula (53) gives $\operatorname{Erfc} x$ conveniently. For intermediate values the three formulæ (53), (54), and (55) are all inappropriate. It was this difficulty of calculation which induced Kramp to compute his Tables. Speaking of the series (53) and (55), he remarks:—" Les deux séries laissent donc entr'elles un vuide pour l'évaluation exacte de l'intégrale, qui peut aller depuis $x = \frac{1}{4}$ jusqu'à $x = 4$, et que nous ne pouvons remplir par aucune des méthodes connues. Comme la connoissance de cette intégrale est absolument essentielle pour le calcul des réfractions qui approchent de l'horizon; comme elle est également indispensable dans l'analyse de hasards; comme de plus la solution d'une infinité d'équations différentielles revient à cette même intégrale, j'ai cru qu'il valoit la peine d'en calculer la table depuis $x = 0.01$ jusqu'à $x = 3.00$" (*Traité des Réfractions*, p. 134). The Tables which Kramp calculated are three in number. Table I. gives $\operatorname{Erf} x$ from $x = 0$ to $x = 1.24$ to eight decimal places, from $x = 1.24$ to $x = 1.50$ to nine places, from $x = 1.50$ to $x = 2.00$ to ten places, from $x = 2.00$ to $x = 3.00$ to eleven places, the intervals throughout being ·01. Differences as far as Δ^3 are added from $x = 0$ to $x = 1.61$, and as far as Δ^4 from $x = 1.61$ to $x = 3.00$. Table II. contains $\log_{10}(\operatorname{Erf} x)$ from $x = 0$ to $x = 3.00$ at intervals of ·01 to seven figures. First and second differences are added. Table III. gives $\log_{10}(e^{x^2} \operatorname{Erf} x)$ from $x = 0$ to $x = 3.00$ at intervals of ·01 to seven figures. First and second differences are added. Table I. was calculated by

means of the formula

$$\text{Erf}\,(x+h) - \text{Erf}\,x = -he^{-x^2}\left\{ 1 - xh + \frac{2x^2-1}{3}h^2 - \frac{2x^3-3x}{6}h^3 \right.$$
$$\left. + \frac{4x^4-12x^2+3}{30}h^4 - \dots \right\}, \quad \dots \quad \dots \quad (60)$$

obtained at once by Taylor's theorem; h was $= {\cdot}01$, and the term in h^5 (*i. e.* the last written above) was small enough to be neglected. Kramp does not state what value he started from in applying the differences, or what means of verification he adopted. In all cases where a Table is constructed by means of differences, the last value should be calculated independently, and then the agreement of the two values would verify all the preceding portion of the Table. This, however, Kramp does not appear to have done, though it would not, as Kramp intimates, have been impossible. To calculate Erfc 3 from (53) would have been a heavy, but by no means exceptionally heavy piece of work. The remark that the "void" cannot be filled up by any known method is not now true. Laplace's continued fraction gives with very little trouble $\text{Erf}\,3 = 000\,019\,577\,193\dots$ This value of Erf 3 differs in the tenth and eleventh figures from Kramp's result obtained by differences; so that it is probable that a portion of his Table is incorrect in the last two figures. I have not yet examined the cause of the error so as to be able to state whether it is due to an inaccuracy in the calculation of a difference, or to an accumulation of small errors in the differences; but I hope shortly to be able to make a complete examination of Kramp's Table.

The next Tables of the functions which were published are due to Bessel; they occupy pages 36, 37 of the *Fundamenta Astronomiæ*, Regiomonti, 1818. Bessel remarks that Kramp's Table does not go beyond $x=3$, and that in a few cases the function might be wanted for larger arguments; he therefore tabulates $e^{x^2}\,\text{Erf}\,x$ for the argument $\log_{10}x$ from 0 to 1 (so that the limits of x are 1 and 10). Bessel gives two Tables. Table I. contains $\log_{10}(e^{x^2}\,\text{Erf}\,x)$ from $x=0$ to $x=1$, at intervals of $\cdot01$, to seven figures, with first and second differences, and is the same as Kramp's[*]. Table II. gives $\log_{10}(e^{x^2}\,\text{Erf}\,x)$, corresponding to the argument $\log_{10}x$, the arguments increasing from 0 to 1 at intervals of $\cdot01$. First and second differences are added. It is not stated how this Table was calculated; the values correspond-

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[*] Bessel probably recomputed the Table, as several of his values differ from Kramp's in the last figure.

ing to arguments between 0 and 3 were probably interpolated from Kramp's Table, and the rest calculated from (55).

A portion* of Legendre's *Traité des Fonctions Elliptiques* (Paris, 1826) is devoted to the discussion of indefinite integrals of the form $\int \left(\log \frac{1}{x} \right)^{a-1} dx$. Legendre writes

$$\Gamma(a, x) = \int_0^x \left(\log \frac{1}{x} \right)^{a-1} dx ; \quad \ldots \quad (61)$$

so that

$$\Gamma(a, e^{-x}) = \int_x^\infty e^{-v} v^{a-1} dv, \quad \ldots \quad \ldots \quad (62)$$

and therefore

$$\tfrac{1}{2}\Gamma(\tfrac{1}{2}, e^{-x^2}) = \text{Erf. } x.$$

Two Tables are given (pp. 520 and 521). Table I. contains $2 \text{ Erf } x$ from $x = 0$ to $x = \cdot50$ at intervals of $\cdot01$, to ten decimal places. The results in this Table should be double of those in the corresponding Table of Kramp's. I have not yet examined if this is the case throughout. This Table was calculated from (53). Table II. gives $\int_0^x dx \left(\log \frac{1}{x} \right)^{a-1}$ from $x = \cdot80$ to $x = 0 \cdot 00$ at intervals of $\cdot01$; that is, it gives $2 \text{ Erf} \left(\log \frac{1}{x} \right)^{\frac{1}{2}}$ from $x = 0$ to $x = \cdot80$. This Table begins about where Table I. ends; so that together they extend from Erf 0 to about Erf (2·14). It was calculated by quadratures, with the exception of the last five or six values, which were obtained from the continued fraction.

At the end of the *Berliner Astronomisches Jahrbuch* for 1834 is a paper by Encke, on the method of least squares, to which are appended two Tables. Table I. gives $\frac{2}{\sqrt{\pi}} \text{ Erfc } x$ from $x = 0$ to $x = 2 \cdot 00$ at intervals of $\cdot01$, to seven places, with first and second differences. Table II. gives $\frac{2}{\sqrt{\pi}} \text{ Erfc } (\rho x)$ from $x = 0$ to $x = 3 \cdot 40$ at intervals of $\cdot01$, and from $x = 3 \cdot 40$ to $x = 5 \cdot 00$ at intervals of $\cdot1$, to five decimal places, with first difference, ρ being determined by the equation $\frac{2}{\sqrt{\pi}} \text{ Erfc } \rho = \tfrac{1}{2}$, so that $\rho = \cdot476\,9360$. The use of this Table is explained by De Morgan

* Chapter xvii. vol. ii.

(*Encyc. Metropol.* "Theory of Probabilities," p. 451).   De
Morgan remarks that he does not know whether this Table was
calculated independently or depends on Kramp's Table ; but on
p. 269 of the *Jahrbuch* Encke says that Table I. was deduced
from Bessel's Table of $\int e^{-x^2}dx$ in the *Fundamenta*.   Bessel, as
we have seen, tabulated $\log_{10}(e^{x^2}\,\mathrm{Erf}\,x)$; and if Encke's Table
was derived from Bessel's, it must have been by interpolation
from his Table II.   It is more probable, however, that it was
derived from Kramp's Table I., from which it can be deduced
at once.   Table II., Encke states, was formed by interpolation,
and is probably founded on Table I.   Both these Tables, as well
as Kramp's Tables I. and II., are reprinted at the end of De
Morgan's article, previously referred to, in the *Encyclopædia
Metropolitana*.

The Table accompanying the present paper gives $\mathrm{Erf}\,x$ from
$x=3{\cdot}00$ to $x=3{\cdot}50$ to eleven places, from $x=3{\cdot}50$ to $x=4{\cdot}00$
to thirteen places, from $x=4{\cdot}00$ to $x=4{\cdot}50$ to fourteen places,
subject to certain qualifications with regard to the accuracy of
the last figure, which will be stated further on.   The values
were calculated by means of the same difference-formula, viz.
(60), that Kramp used.   Separate Tables of $\log_{10} e^{-x^2}(=-x^2\mu,$
μ being the modulus) and of

$$\log_{10}\left\{ h-h^2x+\frac{2x^2-1}{3}\,h^3-\frac{2x^3-3x}{6}\,h^4\right\}$$

were formed.   The second of these Tables was differenced
throughout, and the gradual change of the differences from
$\cdot0000430$ to $\cdot0000427$ afforded a very good test of its accuracy.
A Table of $\mathrm{Erf}\,(x+h)-\mathrm{Erf}\,x$ was then deduced from the two
subsidiary Tables, and was differenced throughout as far as Δ^3;
and the regularity of these last differences proved the correctness
of the Table. $\mathrm{Erf}\,3$ was calculated, as previously mentioned,
from the continued fraction and the differences subtracted from
it, till $\mathrm{Erf}\,3{\cdot}5$ was obtained $=\cdot000\,000\,658\,5487\ldots$ The correct
value obtained from the continued fraction was

$$\mathrm{Erf}\,3{\cdot}5=\cdot000\,000\,658\,553\,76\ldots,$$

so that eleven figures are the same in both ; it is probable, there-
fore, that the last figure in the values from $x=3{\cdot}00$ to $x=3{\cdot}50$
is nowhere in error by so much as a unit.   As an additional
verification, $\mathrm{Erf}\,3{\cdot}2$ was calculated from the continued fraction
and found $=\cdot000\,005\,340\,191\ldots$, which agrees with the num-
ber given in the Table. $\mathrm{Erf}\,4$ was calculated from the continued

fraction and found $= \cdot 000\,000\,013\,663\,189\ldots$; the differences corresponding to argument between 4·0 and 3·5 were then added, and Erf 3·5 was obtained $= \cdot 000\,000\,658\,553\,74\ldots$, agreeing with the value found by direct calculation for Erf 3·5 as far as the thirteenth figure. The values from $x = 3\cdot5$ to $x = 4\cdot0$ were therefore entered in the Table to thirteen decimal places; and it is not probable that any is in error by more than a unit in the last place in any of the values. Starting again from 4·0, the differences corresponding to arguments between 4·0 and 4·5 were subtracted, and Erf 4·5 was thus obtained $= \cdot 000\,000\,000\,174\,250\ldots$ By direct calculation from the continued fraction Erf 4·5 was found $= \cdot 000\,000\,000\,174\,237\,6\ldots$ As these values only differ by unity in the fourteenth place, the portion of the Table from $x = 4\cdot0$ to $x = 4\cdot5$ was given to fourteen places, it being understood that the last figure may be in error to the extent of one or even two units.

From the above description it will be seen that the mode of verification adopted formed a very perfect test of the accuracy of the Table. I rather regret now that I did not tabulate $e^{x^2}\,\mathrm{Erf}\,x$ in preference to $\mathrm{Erf}\,x$. When a function whose value is numerically small enters into analysis, the term involving it can usually be neglected, unless it contains also a very large factor, which is usually an exponential. It is also a matter of observation that when a function is expansible in a descending series multiplied by an exponential, this factor points out the factor which it will be convenient to multiply the function by previously to tabulation; thus from (55) we see that $e^{x^2}\,\mathrm{Erf}\,x$ is a function which only becomes infinitely small for very large values of x. Similarly when x is large, it is preferable to tabulate $e^{-x}\,\mathrm{Ei}\,x$ and $e^x\,\mathrm{Ei}(-x)$ in place of $\mathrm{Ei}\,x$ and $\mathrm{Ei}\,(-x)$. This deficiency in the case of the error-function I hope to supply by forming a Table of $e^{x^2}\,\mathrm{Erf}\,x$ for arguments above 3.

Since $\mathrm{Erfc}\,x = \frac{1}{2}\sqrt{\pi} - \mathrm{Erf}\,x$, the value of $\frac{1}{2}\sqrt{\pi}$ to fourteen places of decimals is printed on the same page as the Tables, to facilitate the deduction of $\mathrm{Erfc}\,x$ from $\mathrm{Erf}\,x$.

TABLE of the Values of the Error-function.

x.	Erf x.	x.	Erf x.	x.	Erf x.
	·0000		·000000		·0000000
3·00	1957719	3·51	6123403	4·01	1258171
3·01	1837943	3·52	5692599	4·02	1158358
3·02	1725164	3·53	5291082	4·03	1066256
3·03	1618995	3·54	4916932	4·04	0981285
3·04	1519069	3·55	4568356	4·05	0902910
3·05	1425037	3·56	4243670	4·06	0830633
3·06	1336570	3·57	3941298	4·07	0763993
3·07	1253354	3·58	3659762	4·08	0702562
3·08	1175095	3·59	3397679	4·09	0645945
3·09	1101510	3·60	3153753	4·10	0593775
3·10	1032335	3·61	2926773	4·11	0545712
3·11	0967319	3·62	2715602	4·12	0501441
3·12	0906224	3·63	2519181	4·13	0460673
3·13	0848824	3·64	2336513	4·14	0423136
3·14	0794907	3·65	2166671	4·15	0388582
3·15	0744272	3·66	2008786	4·16	0356780
3·16	0696729	3·67	1862045	4·17	0327517
3·17	0652097	3·68	1725689	4·18	0300596
3·18	0610207	3·69	1599008	4·19	0275834
3·19	0570898	3·70	1481339	4·20	0253062
3·20	0534019	3·71	1372062	4·21	0232124
3·21	0499426	3·72	1270601	4·22	0212878
3·22	0466984	3·73	1176414	4·23	0195189
3·23	0436566	3·74	1088998	4·24	0178936
3·24	0408050	3·75	1007881	4·25	0164003
3·25	0381324	3·76	0932626	4·26	0150288
3·26	0356279	3·77	0862823	4·27	0137692
3·27	0332816	3·78	0798089	4·28	0126128
3·28	0310837	3·79	0738068	4·29	0115512
3·29	0290254	3·80	0682429	4·30	0105769
3·30	0270982	3·81	0630862	4·31	0096829
3·31	0252941	3·82	0583078	4·32	0088627
3·32	0236056	3·83	0538809	4·33	0081104
3·33	0220255	3·84	0497804	4·34	0074206
3·34	0205472	3·85	0459830	4·35	0067880
3·35	0191644	3·86	0424671	4·36	0062082
3·36	0178713	3·87	0392123	4·37	0056768
3·37	0166622	3·88	0362000	4·38	0051899
3·38	0155319	3·89	0334126	4·39	0047438
3·39	0144754	3·90	0308338	4·40	0043352
3·40	0134883	3·91	0284485	4·41	0039610
3·41	0125660	3·92	0262427	4·42	0036184
3·42	0117045	3·93	0242032	4·43	0033048
3·43	0109000	3·94	0223178	4·44	0030178
3·44	0101488	3·95	0205753	4·45	0027552
3·45	0094476	3·96	0189651	4·46	0025149
3·46	0087931	3·97	0174776	4·47	0022952
3·47	0081824	3·98	0161036	4·48	0020942
3·48	0076126	3·99	0148347	4·49	0019105
3·49	0070811	4·00	0136632	4·50	0017425
3·50	0065855				

$$\text{Erfc}\, x = \tfrac{1}{2}\sqrt{\pi} - \text{Erf}\, x.$$

$$\tfrac{1}{2}\sqrt{\pi} = ·886\ 226\ 925\ 452\ 75 \ldots$$

LV. *Note on some Definite Integrals.*
By R. Pendlebury, *St. John's College, Cambridge*[*].

IN a recent communication to the Philosophical Magazine[†], Mr. Glaisher pressed the claims of the integral

$$\int_x^\infty \epsilon^{-x^2} dx \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

for admission into the rather too scanty family of known functions, and gave a tolerably long list of integrals expressible linearly by this function. The object of this note is to point out that a fresh set of integrals can be found expressible by the squares, cubes, and higher powers of the integral (1). With Mr. Glaisher's notation,

$$\int_x^\infty \epsilon^{-x^2} dx = \text{Erf}(x),$$

I write also

$$\int_0^x \epsilon^{-x^2} dx = \text{erf}(x) ;$$

so that

$$\text{Erf}(x) + \text{erf}(x) = \frac{\sqrt{\pi}}{2}. \quad . \quad . \quad . \quad . \quad (2)$$

Now

$$\begin{aligned}
\text{erf}(\alpha) \, \text{erf}(\beta) &= \int_0^\alpha \epsilon^{-x^2} dx \int_0^\beta \epsilon^{-y^2} dy \\
&= \int_0^\alpha \int_0^\beta \epsilon^{-(x^2+y^2)} dx \, dy \\
&= \iint \rho \epsilon^{-\varsigma^2} d\rho \, d\theta,
\end{aligned}$$

the integral being transformed by the equations $x = \rho \cos \theta$, $y = \rho \sin \theta$. The new double integral divides into two, with different limits; viz.

$$\rho = \left.\begin{matrix} \alpha \sec \theta \\ 0 \end{matrix}\right\} \quad \theta = \left.\begin{matrix} \arctan \dfrac{\beta}{\alpha} \\ 0 \end{matrix}\right\},$$

$$\rho = \left.\begin{matrix} \beta \csc \theta \\ 0 \end{matrix}\right\} \quad \theta = \left.\begin{matrix} \dfrac{\pi}{2} \\ \arctan \dfrac{\beta}{\alpha} \end{matrix}\right\}.$$

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* Communicated by the Author.
† Philosophical Magazine, October 1871.

We get then readily, integrating by parts,

$$\operatorname{erf}(\alpha)\operatorname{erf}(\beta) = \frac{\pi}{4} - \frac{1}{2}\int_0^{\arctan\frac{\beta}{\alpha}} \epsilon^{-\alpha^2\sec^2\theta}\,d\theta - \frac{1}{2}\int_{\arctan\frac{\beta}{\alpha}}^{\frac{\pi}{2}} \epsilon^{-\beta^2\operatorname{cosec}2\theta}. \quad (3)$$

Putting in the first integral on the right hand $\tan\theta = x$, and in the second $\cot\theta = x$,

$$\operatorname{erf}(\alpha)\operatorname{erf}(\beta) = \frac{\pi}{4} - \frac{1}{2}\epsilon^{-\alpha} \int_0^{\frac{\beta}{\alpha}} \frac{\epsilon^{-\alpha^2 x^2}}{1+x^2}\,dx - \frac{1}{2}\epsilon^{-\beta^2}\int_0^{\frac{\alpha}{\beta}} \frac{\epsilon^{-\beta^2 x^2}}{1+x^2}\,dx; \quad (4)$$

and, in particular, when $\alpha = \beta$,

$$\{\operatorname{erf}(\alpha)\}^2 = \frac{\pi}{4} - \epsilon^{-\alpha^2}\int_0^1 \frac{\epsilon^{-\alpha^2 x^2}\,dx}{1+x^2}. \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

In combination with the last equation may be used the equation (5) of Mr. Glaisher's paper quoted above, which gives

$$\sqrt{\pi}\,\operatorname{Erf}(\alpha) = \epsilon^{-\alpha^2}\int_0^\infty \frac{\epsilon^{-\alpha^2 x^2}\,dx}{1+x^2}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (6)$$

Putting, in (5), $\operatorname{erf}\alpha = \frac{\sqrt{\pi}}{2} - \operatorname{Erf}(\alpha)$, we get

$$-\sqrt{\pi}\,\operatorname{Erf}(\alpha) + \{\operatorname{Erf}(\alpha)\}^2 = -\epsilon^{-\alpha^2}\int_0^1 \frac{\epsilon^{-\alpha^2 x^2}\,dx}{1+x^2};$$

$$\therefore \{\operatorname{Erf}(\alpha)\}^2 = \epsilon^{-\alpha^2}\int_1^\infty \frac{\epsilon^{-\alpha^2 x^2}\,dx}{1+x^2}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (7)^*$$

If we multiply both sides of (7) by $d\alpha$ and integrate between the limits $\begin{cases} \infty \\ 0 \end{cases}$, we get the curious result,

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* The equation (7) can be obtained directly without much difficulty. For

$$\operatorname{Erf}(\alpha) = \int_\alpha^\infty \epsilon^{-x^2}\,dx = \int_1^\infty \alpha\epsilon^{-\alpha^2 x^2}\,dx;$$

$$\therefore \epsilon^{-\alpha^2}\operatorname{Erf}(\alpha) = \int_1^\infty \alpha\epsilon^{-\alpha^2(1+x^2)}\,dx;$$

$$\therefore \operatorname{Erf}(\alpha)\frac{d\operatorname{Erf}(\alpha)}{d\alpha} = -\int_1^\infty \alpha\epsilon^{-\alpha^2(1+x^2)}\,dx;$$

and integrating,

$$\{\operatorname{Erf}(\alpha)\}^2 = \int_1^\infty \frac{\epsilon^{-\alpha^2(1+x^2)}\,dx}{1+x^2},$$

the constant of integration being zero.

$$\int_0^\infty \{\mathrm{Erf}\,(\alpha)\}^2 d\alpha = \int_1^\infty \frac{dx}{1+x^2}\int_0^\infty \epsilon^{-\alpha^2(1+x^2)}\,d\alpha$$

$$= \frac{\sqrt{\pi}}{2}\int_1^\infty \frac{dx}{(1+x^2)^{\frac{3}{2}}}$$

$$= \frac{\sqrt{\pi}}{2}\left(1-\frac{1}{\sqrt{2}}\right). \quad . \quad . \quad . \quad (8)$$

It is clear now that, by the various transformations to which the integral $\int_0^1 \frac{\epsilon^{-\alpha^2 x^2}\,dx}{1+x^2}$ can be subjected, we have a new series of definite integrals opened out, which may perhaps be worth the trouble of arrangement and tabulation.

The fourth power of the Error-function can be easily expressed as a single integral by the method adopted to determine the value of the square.

We easily get from (7),

$$[\mathrm{Erf}\,(\alpha)]^4 = \epsilon^{-\alpha^2}\int_1^\infty \frac{\epsilon^{-\alpha^2 x^2}\,dx}{1+x^2}\ \epsilon^{-\alpha^2}\int_1^\infty \frac{\epsilon^{-\alpha^2 y^2}\,dy}{1+g^2}$$

$$= \epsilon^{-2\alpha^2}\int_1^\infty\int_1^\infty \frac{\epsilon^{-\alpha^2(x^2+y^2)}\,dx\,dy}{1+x^2+y^2+x^2y^2}$$

$$= \epsilon^{-2\alpha^2}\iint \frac{\rho\epsilon^{-\alpha\,\varrho}}{1+\rho^2+\rho^4\sin^2\theta\cos^2\theta}\cdot d\rho\,d\theta$$

$$= \frac{1}{2}\,\epsilon^{-2\alpha^2}\int \frac{\rho\epsilon^{-\alpha^2\varrho^2}}{\sqrt{1+\rho^2}\left(1+\dfrac{\rho^2}{2}\right)}\left[\mathrm{arc\ tan}\ \frac{1+\dfrac{\rho^2}{2}}{\sqrt{1+\rho^2}}\cdot\tan 2\theta\right]d\rho.$$

The limits are

(i) $\quad \theta = \begin{matrix}\mathrm{arc\ tan}\ \sqrt{\rho^2-1}\\0\end{matrix}\Big\}\rho = \begin{matrix}\sqrt{2}\\1\end{matrix}\Big\},$

(ii) $\quad \theta = \begin{matrix}\dfrac{\pi}{2}\\\mathrm{arc\ cot}\ \sqrt{\rho^2-1}\end{matrix}\Big\}\ \rho = \begin{matrix}\sqrt{2}\\1\end{matrix}\Big\},$

(iii) $\quad \theta = \begin{matrix}\dfrac{\pi}{2}\\0\end{matrix}\Big\}\ \ \rho = \begin{matrix}\infty\\\sqrt{2}\end{matrix}\Big\}.$

The last of these three terms vanishes. The others are equal, and give

$$\{\mathrm{Erf}\,(\alpha)\}^4$$
$$=\epsilon^{-2\alpha^2}\int_1^{\sqrt{2}}\frac{2\rho\epsilon^{-\alpha^2\varsigma^2}}{\sqrt{1+\rho^2}\,(2+\rho^2)}\ \mathrm{arc\ tan}\left(\frac{2+\rho^2}{\sqrt{1+\rho^2}}\cdot\frac{\sqrt{\rho^2-1}}{2-\rho^2}\right)d\rho,$$

or, putting $\rho^2=x$,

$$\{\mathrm{Erf}\,(\alpha)\}^4$$
$$=\epsilon^{-2\alpha^2}\int_1^2\frac{\epsilon^{-\alpha^2\cdot x}}{\sqrt{1+x}\cdot(x+2)}\ \mathrm{arc\ tan}\left(\frac{x+2}{2-x}\sqrt{\frac{x-1}{x+1}}\right)dx.\ \ .\ \ (9)$$

If we differentiate (9) with respect to α, we obtain an expression for $(\mathrm{Erf}\,\alpha)^3$ as a definite integral, viz. :—

$$4\{\mathrm{Erf}\,(\alpha)\}^3=2\alpha\int_1^2\frac{\epsilon^{-\alpha^2(x+1)}}{\sqrt{1+x}}\ \mathrm{arc\ tan}\left(\frac{x+2}{2-x}\sqrt{\frac{x-1}{x+1}}\right)dx.\ \ (10)$$

Of the three formulæ (7), (9), (10), the first may open a series of interesting integrals; the other two are perhaps too complex to be any thing but a matter of curiosity.

Cambridge, October 31, 1871.

LVI. *On a Problem in the Calculus of Variations.* By Isaac Todhunter, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN the Philosophical Magazine for July Professor Challis has discussed three problems in the Calculus of Variations. He states, in connexion with the first problem, that a certain conclusion obtained by Legendre and Stegmann, and tacitly accepted by myself, is erroneous.

There is, however, no error. Professor Challis does not understand the problem in the same sense as Legendre and Stegmann. I have enunciated the problem thus :—required to connect two fixed points by a curve of given length so that the area bounded by the curve, the ordinates of the fixed points, and the axis of abscissæ shall be a maximum. It is intended that the curve should be *confined between the indefinite straight lines* which coincide in position with the extreme ordinates. The enunciation involves this condition; for otherwise nothing would be gained by introducing the *ordinates* of the fixed points. And the investigation given would show, if there were any doubt, that this is the precise meaning intended.

It is in fact this condition which constitutes the chief interest of the problem; and there can be no doubt that the solution of Legendre and Stegmann is correct. Stegmann's investigation

seems to have been independent of Legendre's; and I had my-self arrived at the same result before I had seen what these mathematicians had written on the subject. See pages ix and 427 of my ' History of the Calculus of Variations.'

The problem as discussed by Professor Challis is free from this condition; it may be enunciated thus :—required to connect the ends of a fixed straight line by a curve of given length so that the area bounded by the curve and the straight line may be a maximum. The result is well known, namely that the curve must be an arc of a circle. The given length must, of course, be greater than the length of the fixed straight line; Professor Challis by a misprint has *less* instead of *greater*. The problem as thus enunciated is one of the oldest and most familiar in the subject; and I believe there has never been any doubt or diffi-culty as to the result, which may be obtained by various unex-ceptionable methods; I have indicated one of these methods at page 69 of my ' History.'

I do not accept the results at which Professor Challis arrives with respect to the other two problems he discusses; but I have not leisure to enter into details. I have, I believe, fully solved these problems in an Essay which will be published in the course of the present month.

I. Todhunter.

St. John's College, Cambridge,
November 6, 1871.

LVII. *On a Correction sometimes required in Curves professing to represent the Connexion between two Physical Magnitudes. By the* Hon. J. W. Strutt, *M.A.*

THE nature of the correction which is the subject of the present paper, and of not infrequent application in ex-perimental inquiry, will be best understood from an example, as it is a little difficult to state with full generality. Suppose that our object is to determine the distribution of heat in the spec-trum of the sun or any other source of light. A line thermo-pile would be placed in the path of the light, and the deflection of the galvanometer noted for a series of positions. But the observations obtained in this way are not *sharp*—that is, they do not correspond to *definite* values of the wave-length or refrac-tive index. In the first place, the spectrum cannot be absolutely pure; at each point there is a certain admixture of neighbouring rays. Further, even if the spectrum were pure, it would still be impossible to operate with a mathematical line of it; so that the result, instead of belonging to a simple definite value of the

* Communicated by the Author.

independent variable, is really a kind of average corresponding to values grouped together in a small cluster.

For the sake of simplicity, let us suppose that the spectrum is originally pure, and that the true curve giving the relations between the two quantities is P Q R. Also let M N be the range over which the independent variable changes in each observation —in our case the width of the thermopile. Then the observed curve is to be found from the true by taking m, the middle point of M N, and erecting an ordinate $p\,m$, such that

$$p\,m \,.\, \mathrm{M\,N} = \text{area of curve P Q N M}.$$

The locus of p will give the curve expressing the result of the observations. It remains to find a convenient method of passing from the one curve to the other.
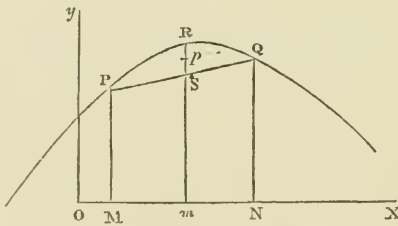
In the figure P R Q represents the true curve, M N the *range* as before; $\mathrm{M}\,m = m\,\mathrm{N} = h$; p is the point on the observed curve found in the manner described; $\mathrm{O}\,m = x_0$, $\mathrm{R}\,m = y_0$, $p\,m = y'$. Now

$$\text{area M P R Q N M} = \int_{-h}^{+h} y\,dh$$

$$= \int_{-h}^{+h} \left(y_0 + \frac{dy}{dx_0}h + \tfrac{1}{2}\frac{d^2y}{dx_0^2}h^2 \right) dh$$

$$= 2h\left(y_0 + \frac{h^2}{6}\frac{d^2y}{dx_0^2} \right).$$

Thus

$$y' = pm = y_0 + \frac{h^2}{6}\frac{d^2y}{dx_0^2}, \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{A})$$

which shows how to deduce y' from y.

To pass backwards, we observe that

$$\frac{d^2y'}{dx^2} = \frac{d^2y}{dx^2} + \frac{h^2}{6}\frac{d^4y}{dx};$$

$$\therefore \frac{h^2}{6}\frac{d^2y}{dx^2} = \frac{h^2}{6}\left(\frac{d^2y'}{dx^2} - \frac{h^2}{6}\frac{d^4y}{dx^4} \right) = \frac{h^2}{6}\frac{d^2y'}{dx^2},$$

if h^4 be neglected. Thus

$$y = y' - \frac{h^2}{6} \frac{d^2 y}{dx^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B)}$$

(A) and (B) give the analytical solution of the problem; but for practical purposes the following interpretation is important:

$$\mathrm{S}m = \frac{1}{2} \left\{ y_0 - \frac{dy}{dx_0} h + \frac{d^2 y}{dx_0^2} \frac{h^2}{2} \right\} + \frac{1}{2} \left\{ y_0 + \frac{dy}{dx_0} h + \frac{d^2 y}{dx_0^2} \frac{h^2}{2} \right\}$$

$$= y_0 + \frac{d^2 y}{dx_0^2} \frac{h^2}{2};$$

so that

$$y' = y_0 - \tfrac{1}{3} \mathrm{R} \, \mathrm{S}.$$

In passing from the observed to the true curve, the curvature is everywhere to be increased instead of diminished, and

$$p \, m = \mathrm{R} \, m + \tfrac{1}{3} \mathrm{R} \, \mathrm{S}.$$

It may be remarked that while it is always possible to pass accurately from the true curve to that derived from it with any prescribed range, the inverse problem is not determinate unless it be understood that the range is small, so that its fourth power may be neglected. The practical utility of the solution obtained is scarcely affected by this consideration; indeed it is only when the curvature of the curve is considerable that the correction itself is of much importance.

It often happens that the connexion between the two curves is not so simple, at least at first sight. Suppose, for example, that in the case taken as an illustration the spectrum is impure from the sensible width (2κ) of the image of the slit. The observed curve is then connected with true by a *double* integration,

$$y' = \frac{1}{4h\kappa} \int_{x-\kappa}^{x+\kappa} d\kappa \left\{ \int_{x-h}^{x+h} y \, dh \right\}.$$

Now

$$\frac{1}{2h} \int_{x-h}^{x+h} y \, dh = y + \frac{h^2}{6} \frac{d^2 y}{dx^2},$$

$$\frac{1}{2\kappa} \int_{x-\kappa}^{x+\kappa} y \, d\kappa = y + \frac{\kappa^2}{6} \frac{d^2 y}{dx^2},$$

$$\frac{1}{2\kappa} \int_{x-\kappa}^{x+\kappa} \frac{h^2}{6} \frac{d^2 y}{dx^2} \, d\kappa = \frac{h^2}{6} \left(\frac{d^2 y}{dx^2} + \frac{\kappa^2}{6} \frac{d^4 y}{dx^4} \right) = \frac{h^2}{6} \frac{d^2 y}{dx^2},$$

if the term in $h^2 \kappa^2$ may be neglected. Thus

$$y' = y + \frac{h^2 + \kappa^2}{6} \frac{d^2 y}{dx^2}.$$

The rules remain just as before, except that instead of h we

now have $\sqrt{h^2 + \kappa^2}$. Similarly, when the want of sharpness is due to more than two causes, we must replace h by $\{\Sigma h^2\}^{\frac{1}{2}}$. When, as often happens, the product of the quantities $h\kappa \ldots$ is to be considered as given, the experiments are best arranged so as to make the independent quantities equal; for then the agreement between the two curves is the closest.

The practical rule to which we are led by the considerations explained in this paper is therefore as follows :—

Construct the curve representing the immediate results of the observations in the ordinary way. Let R m *be any ordinate. Draw*
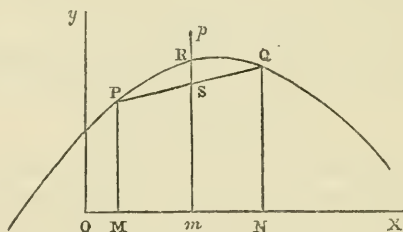
parallels P M, Q N *at distances equal to* h, *or* $\{\Sigma h^2\}^{\frac{1}{2}}$, *and join* P Q *cutting* R m *in* S. *The point* p *on the true curve corresponding to the abscissa* O m *is to be found by taking* p R *equal to one third of* R S, *and so that* p *and* S *lie on opposite sides of* R.

LVIII. *On a Method of Measuring the Lateral Diffusion of a Current in a Conductor by means of Equipotential Lines; being a Note on a Paper by* Dr. Macaluso *"On the Transmission of Electricity in Liquids," in the Philosophical Magazine for November* 1871. *By* J. E. H. GORDON[*].

IN the abstract of Dr. Macaluso's paper given in the Philosophical Magazine for November 1871, p. 389, it is stated that "when an electric current travels through a liquid whose cross section is much greater than the surface of the electrodes, it tends to diffuse itself laterally." About two years ago I made some experiments in the physical laboratory of King's College, on the lateral diffusion of the current in conductors. Instead of liquids, however, I used sheets of tinfoil of various shapes.

I was not only able to experimentally detect the lateral diffusion, but also to arrange a method by which its direction and amount may be measured. The outline of the method was suggested to me by a friend. I do not know whether it is new; but as I have

not seen any description of it, and as many details were arranged by me, I think it may be worth while to publish a short account of it.

The law of the diffusion of the current in a solid conductor is of course perfectly well known. In this paper I merely propose to describe a method of experimentally verifying this known law, and to suggest a modification of it by means of which the (apparently) unknown law for the diffusion of the current in a liquid may probably be determined.

The object of the method was, on a surface of tinfoil through which a current was flowing, to determine a number of groups of points of equal tension, and to draw equipotential lines through them. From these lines the positions of the currents can be easily deduced.

My plan was as follows :—A sheet of tinfoil of any desired shape was pasted on to a hard mahogany board. At the two points chosen for the electrodes holes were drilled, through which screws were passed so that their heads (about 7 millims. in diameter) rested on the tinfoil; t'.. .. stems passed through the board into binding-screws on the unde.... A little mercury was placed under the heads to make better co... \ct. Wires from a battery passed through a contact-breaker to these screws. Two cells of Grove's battery were at first used; but the power was afterwards reduced to one, owing to the great heating-effect of two cells.

The contact-breaker was arranged so that two circuits could be broken or made simultaneously, namely the battery and galvanometer circuits. A current is produced simply by difference of tension. Therefore, if two poles of a sufficiently sensitive galvanometer were placed in such a position on the tinfoil that there was no deflection when the current passed, these two points were in the same equipotential line.

The beautiful reflecting galvanometer belonging to King's College was used, and was found to be so delicate that, after a little practice, a deviation of less than 1 millimetre from the right position on the tinfoil could be easily detected on the galvanometer.

The same plan can be used for determining the lines in liquid conductors by insulating the wires up to their points, and having each galvanometer-terminal fixed on a little stage to slide on the top of the trough, which must be divided so that from the position of the stage the horizontal coordinates of the point of the wire can be determined, while the wire slides through the stage and is divided to give the vertical element.

Below is an engraving of half one of the sheets of tinfoil with the lines traced on it. The sheet is divided by a line at right angles to, and bisecting, the line joining the battery-poles. The

upper half of the sheet is given ; *i. e.* if the whole sheet were given, the second half would lie lower down on the page. The slight irregularities in the curves are no doubt produced by variations in the thickness of the tinfoil.

Breadth =27·6 centims.

Half length =15·5 centims.
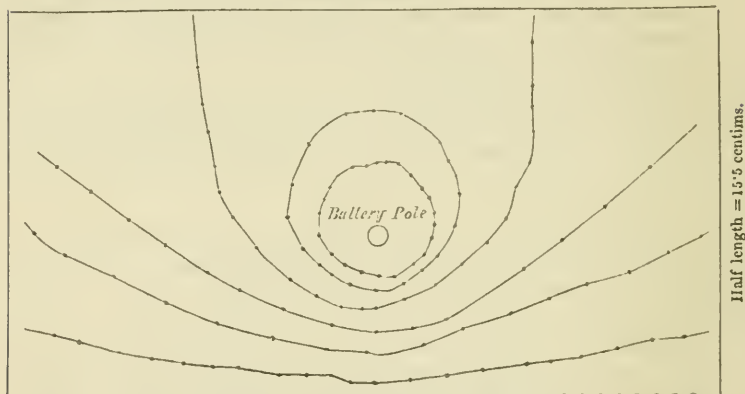
The method of making contact with the tinfoil and marking the centre of the place of contact was follows :—The wires from the terminals of the galvanometer were attached to two thin brass rods with blunt conical points 3 millims. in diameter. α was straight, and had a needle-point projecting about 1 millim. from the centre of its blunt point. α was held in a clip and placed
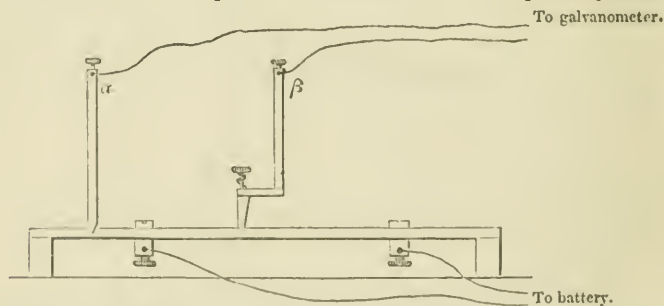
vertically on any part of the tinfoil through which it was desired to trace an equipotential line, so that the whole of the blunt point made contact, and the needle-point made a dot in the centre of the contact. It is now required to move β about on the tinfoil till there is no deflection (*i. e.* till it is in the same equipotential line as α), and then, and not till then, to make a dot in the centre of the contact. The way in which this was managed may be seen by looking at the figure. β is bent twice at right angles, so that it is only about 20 millims. from the.

blunt point to the first bend. A fine hole is drilled from the centre of the blunt point to the bend. In this hole a needle slides; there is a button on the top to press it down by, and a spiral spring to raise it again after being pressed down. In the experiments β was moved about till there was no deflection; the button was then pressed and a dot made. β was then removed to another point, and the process was repeated until a sufficient number of points for one line had been obtained; α was then moved, and another line traced in the same manner.

LIX. *Note on a Spiral Leyden Jar.*
By Frederick Guthrie[*].

A STRIP of tinfoil 4 feet long and 8 inches wide is placed upon a strip of vulcanized caoutchouc 4 feet long and 1 foot wide, in such a way that along both sides there is a margin of two inches of caoutchouc, at one end (say the right) a margin of four inches of caoutchouc, and at the left a margin of four inches of tinfoil. A second piece of caoutchouc exactly similar to the first is placed exactly over the first upon the foil. A second piece of tinfoil of the same width as the first, but four inches shorter, is placed on the second caoutchouc above the first foil, with its right-hand end above the right end of the first foil. Its left end, of course, falls four inches short. A brass wire carrying a knob is laid across the end of the upper foil. The whole is rolled up from the right end and bound. What was the lower of the two foils projects between the two layers of caoutchouc, and may be prolonged around the circumference of the roll. It forms the outer coating or earth-surface. What was the upper coating of foil now forms what corresponds to the inner coating of the ordinary jar, and is entirely covered, excepting where it is prolonged as the wire and knob at the centre of the roll. If the sheet caoutchouc be an eighth of an inch in thickness, a jar of very great electrical capacity is obtained in a very compact form, and one which is free from the risk of fracture, and less impaired than the ordinary jar by atmospheric moisture.

A very serviceable modification of this form of jar has been constructed for me by Mr. W. Peters[†]. The insulating material is sheet ebonite. The construction is similar to the above. The ends of the spiral roll are capped with dry mahogany disks. The earth-foil is connected with a brass girdle around the centre of the cylinder, and does not itself appear on the outside. The electric capacity is between four and five times as great as that of a glass jar of the same volume. It has been in use for several months, and appears almost incapable of receiving injury.

* Communicated by the Author.
† W. Peters, 36 Whiskin Street, Clerkenwell, E.C.

LX. *On the Equilibrium of Vapour at a Curved Surface of Liquid.* *By* Sir WILLIAM THOMSON, *F.R.S.**

IN a closed vessel containing only a liquid and its vapour, all at one temperature, the liquid rests, with its free surface raised or depressed in capillary tubes and in the neighbourhood of the solid boundary, in permanent equilibrium according to the same law of relation between curvature and pressure as in vessels open to the air. The permanence of this equilibrium implies physical equilibrium between the liquid and the vapour in contact with it at all parts of its surface. But the pressure of the vapour at different levels differs according to hydrostatic law. Hence the pressure of saturated vapour in contact with a liquid differs according to the curvature of the bounding surface, being less when the liquid is concave and greater when it is convex. And detached portions of the liquid in separate vessels all enclosed in one containing vessel cannot remain permanently with their free surfaces in any other relative positions than those they would occupy, if there were hydrostatic communication of pressure between the portions of liquid in the several vessels. There will be evaporation from those surfaces which are too high, and condensation into the liquid at those surfaces which are too low—a process which will go on until hydrostatic equilibrium, as if with free communication of pressure from vessel to vessel, is attained. Thus, for example, if there are two large open vessels of water, one considerably above the other in level, and if the temperature of the surrounding matter is kept rigorously constant, the liquid in the higher vessel will gradually evaporate until it is all gone and condensed into the lower vessel. Or if, as illustrated by the annexed diagram, a capillary tube with a small quantity of liquid occupying it from its bottom up to a certain level be placed in the neighbourhood of a quantity of the same liquid with a wide free surface, vapour will gradually become condensed into the liquid in the capillary tube until the level of the liquid in it is the same as it would be were the lower end of the tube in hydrostatic communication with the large mass of liquid. Whether air be present above the free surface of the liquid in the several vessels or not, the condition of ultimate equilibrium is the same; but the processes of evaporation and condensation through which equilibrium is approached will be very much retarded by the presence of air. The experiments of Graham and the kinetic theory of Clausius and Maxwell scarcely yet afford us sufficient data for estimating the rapidity with which the vapour proceeding from one of the liquids will diffuse itself through the air and reach the surface

of another liquid at a lower level. With air at any thing approaching to ordinary atmospheric density to resist the process, it is probable it would be too slow to show any results unless in very long-continued experiments. But if the air be removed as perfectly as can be done by well-known practical methods, it is probable that the process will be very rapid; it would indeed be instantaneous, were it not for the cold of evaporation in one vessel and the heat of condensation in the other. Practically, then, the rapidity of the process towards hydrostatic equilibrium through vapour between detached liquids depends on the rate of the conduction of heat between the several surfaces through intervening solids and liquids. Without having made either the experiment or any calculations on the rate of conduction of heat in the circumstances, I feel convinced that in a very short time water would visibly rise in the capillary tube indicated in the diagram, and that, provided care is taken to maintain equality of temperature all over the surface of the hermetically sealed vessel, the liquid in the capillary tube would soon take very nearly the same level as it would have were its lower end open—sinking to this level if the capillary tube were in the beginning filled too full, or rising to it if (as indicated in the diagram) there were not enough of liquid in it at first to fulfil the condition of equilibrium.
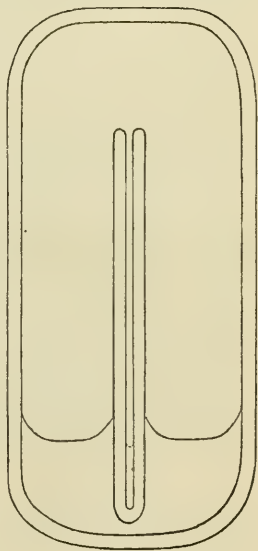
The following formulæ show precisely the relations between curvatures, differences of level, and differences of pressure with which we are concerned.

Let ρ be the density of the liquid and σ that of the vapour, and let T be the cohesive tension of the free surface per unit of breadth, in terms of weight of unit mass as unit of force. Let h denote the height of any point P of the free surface above a certain plane of reference, which I shall call for brevity the plane level of the free surface. This will be sensibly the actual level of the free surface in regions (if there are any) with no part of the edge (or bounding line of the free surface where liquid ends and solid begins) at a less distance than several centimetres. Lastly, let r and r' be the principal radii of curvature of the surface at P. By Laplace's well-known law we have, as the equation of equilibrium,

$$(\rho - \sigma)h = T\left(\frac{1}{r} + \frac{1}{r'}\right). \quad . \quad . \quad . \quad . \quad (1)$$

Now, in the space occupied by vapour, the pressure is less at the higher than at the lower of two points whose difference of levels is h, by a difference equal to σh. And there is permanent equilibrium between vapour and liquid at all points of the free surface. Hence the pressure of vapour in equilibrium is less at a concave than at a plane surface of liquid, and less at a plane surface than at a convex surface, by differences amounting to $\dfrac{T\sigma}{\rho-\sigma}$ per unit difference of curvature. That is to say, if ϖ denote the pressure of vapour in equilibrium at a plane surface of liquid, and p the pressure of vapour of the same liquid at the same temperature presenting a curved surface to the vapour, we have

$$p = \varpi - \frac{T\sigma}{\rho-\sigma}\left(\frac{1}{r}+\frac{1}{r'}\right), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

$\dfrac{1}{r}$ and $\dfrac{1}{r'}$ being the curvatures in the principal sections of the surface bounding liquid and vapour, reckoned positive when concave towards the vapour.

In strictness, the value of σ to be used in these equations, (1) and (2), ought to be the mean density of a vertical column of vapour extending through the height h from the plane of reference. But in all cases to which we can practically apply the formulæ, according to present knowledge of the properties of matter, the difference of densities in this column is very small, and may be neglected. Hence, if H denote the height of an imaginary homogeneous fluid above the plane of reference, which, if of the same density as the vapour at that plane, would produce by its weight the actual pressure ϖ, we have

$$\sigma = \frac{\varpi}{H}.$$

Hence, by (1) and (2),

$$p = \varpi\left(1-\frac{h}{H}\right). \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

For vapour of water at ordinary atmospheric temperatures, H is about 1,300,000 centimetres. Hence, in a capillary tube which would keep water up to a height of 13 metres above the plane level, the curved surface of the water is in equilibrium with the vapour in contact with it, when the pressure of the vapour is less by about $\frac{1}{1000}$ of its own amount than the pressure of vapour in equilibrium at the plane surface of water at the same temperature.

For water the value of T at ordinary temperatures is about ·08 of a gramme weight per centimetre; and ρ, being the mean of a cubic centimetre, in grammes, is unity. The value of σ for vapour of water, at any atmospheric temperature, is so small that we may neglect it altogether in equation (1). In a capillary

tube thoroughly wet with water, the free surface is sensibly he-
mispherical, and therefore r and r' are each equal to the radius
of the inner surface of the liquid film lining the tube above the
free liquid surface; we have therefore

$$h = \cdot 08 \times \frac{2}{r}.$$

Hence, if $h = 1300$ centims., $r = \cdot 00012$ centim., there can be
no doubt that Laplace's theory is applicable without serious
modification, even to a case in which the curvature is so great
(or radius of curvature so small) as this. But in the present
state of our knowledge we are not entitled to push it much fur-
ther. The molecular forces assumed in Laplace's theory to be
" insensible at sensible distances " are certainly but little, if at
all, sensible at distances equal to or exceeding the wave-lengths
of ordinary light. This is directly proved by the most cursory
observation of soap-bubbles. But the appearances presented
by the black spot which abruptly ends the series of colours at
places where the " bubble " is thinnest before it breaks make it
quite certain that the action of those forces becomes sensible at
distances not much less than a half wave-length, or $\frac{1}{40000}$ of a
centimetre. There is indeed much and multifarious evidence that,
in ordinary solids and liquids, not merely the distances of sen-
sible intermolecular action, but the linear dimensions of the
molecules themselves, and the average distance from centre to
nearest centre*, are but very moderately small in comparison
with the wave-lengths of light. Some approach to a definite
estimate of the dimensions of molecules is deducible from Clau-
sius's theory of ten of the average spaces travelled without colli-
sion by molecules of gases, and Maxwell's theory and experi-
ments regarding the viscosity of gases. Having perfect confi-
dence in the substantial reality of the views which these grand
investigations have opened to us, I find it scarcely possible to
admit that there can be as many as ten molecules in a cubic
centimetre of liquid carbonic acid or of water. This makes the
average distance from centre to nearest centre in the liquids
exceed a thousand-millionth of a centimetre !

We cannot, then, admit that the *formulæ* which I have given
above are applicable to express the law of equilibrium between the
moisture retained by vegetable substances, such as cotton cloth or
oatmeal, or wheat-flour biscuits, at temperatures far above the dew-
point of the surounding atmosphere. But although the energy
of the attraction of some of these substances for vapour of water
(as exemplified when oatmeal, previously dried at a high tempe-
rature, has been used instead of sulphuric acid to produce the

* By " average distance from centre to nearest centre," I mean the side
of the cube in a cubic arrangement of a number of points equal to the number
of real molecules in any space.

freezing of water under the reciever of an air-pump) is so great that it might almost claim recognition from chemists as due to a " chemical affinity," and resulting in a " chemical combination," I believe that the absorption of vapour into fibrous and cellular organic structures is a property of matter continuous with the absorption of vapour into a capillary tube demonstrated above.

LXI. *On the Reflection and Refraction of Light by thin Layers of Metal.* By E. Jochmann[*].

THE optical properties of thin metallic lamellæ have recently been subjected by Quincke to a series of careful and in many ways varied experimental investigations[†]. The remarkable and in part unexpected results of these investigations make sensible a deficiency in the theory of reflection, in that the theory of the interference-phenomena of thin transparent lamellæ has not hitherto been extended to metallic media. Starting from the principles of Cauchy's theory of reflection, I arrived at a system of formulæ which represent the phenomena of reflection and refraction of light by thin metallic lamellæ with the same degree of mathematical accuracy as the formulæ of Cauchy for metallic reflection on the supposition of a considerable thickness of the metallic layer. Cauchy's formulæ for the intensity and phase of light polarized perpendicular to the plane of incidence include, as is known, the simplifying presupposition that the coefficient of ellipticity ϵ (originating from the influence of the longitudinal waves) may be put equal to 0. We know that the influence of this coefficient with transparent media is always but very little, except in the vicinity of the angle of polarization. With metals there is no polarization-angle in the same sense as with transparent media ; and indeed the comparison of Cauchy's formulæ with experiment teaches that the influence of the coefficient of ellipticity may, without sensible error, be neglected, even with metallic media. So much the more will this presupposition be justified when the theory is applied to thin metallic lamellæ, in which manifold causes cooperate diminishing the accuracy attainable by the measurements. Although the method which I have used for the derivation of the following formulæ permits the carrying out in full rigour the solution of the problem, yet it appeared useful, in the case of light polarized perpendicular to the plane of incidence, to introduce that simplifying presupposition, in order to give to the formulæ a form applicable to the

derivation of consequences practicable and suitable for comparison with experiment.

The three media to be considered shall, according to the order of succession, be designated as the 1st, 2nd, and 3rd; and the quantities referring to them shall have the corresponding indices 1, 2, 3. Accordingly let λ_1 and λ_3 be the wave-lengths, α_1 and α_3 the angle of incidence in the first and the angle of refraction in the third medium. The sines and cosines of these angles will, for the sake of brevity, be denoted by s_1, c_1, s_3, c_3. The wave-length λ_2 and the angle of refraction α_2, for the second, metallic medium, are, it is well known, according to the notation introduced by Cauchy, imaginary. If, then, we put

$$\lambda_2 = l_2 e^{-\epsilon i}$$

and denote the complex values of $\sin \alpha_2$ and $\cos \alpha_2$ by

$$s_2 e^{-\epsilon i}, \quad c_2 e^{u i},$$

in which l_2, s_2, c_2 are real positive quantities, and ϵ, u real angles lying between 0 and $\frac{\pi}{2}$, it is well known we shall have

$$\frac{s_1}{\lambda_1} = \frac{s_2}{l_2} = \frac{s_3}{\lambda_3},$$

and the complex refraction-ratios for the passage out of the first or third into the second medium will be expressed by $\theta_1 e^{\epsilon i}$ or $\theta_3 e^{\epsilon i}$, where

$$\theta_1 = \frac{s_1}{s_2} = \frac{\lambda_1}{l_2}, \quad \theta_3 = \frac{s_3}{s_2} = \frac{\lambda_3}{l_2}.$$

The quantities s_2, c_2, ϵ, u are therein connected by the relations

$$s_2^2 \cos 2\epsilon + c_2^2 \cos 2u = 1,$$
$$s_2^2 \sin 2\epsilon - c_2^2 \sin 2u = 0,$$

which are always to be identically fulfilled.

For abbreviation, further, for $\nu = 1$ or 3, let

$$q_\nu = \frac{c_2 s_\nu}{c_\nu s_2}, \qquad\qquad \mathfrak{q}_\nu = \frac{c_\nu s_\nu}{c_2 s_2};$$

$$U_\nu = 1 + 2q_\nu \cos(\epsilon + u) + q_\nu^2, \quad \mathfrak{U}_\nu = 1 + 2\mathfrak{q}_\nu \cos(\epsilon - u) + \mathfrak{q}_\nu^2;$$

$$V_\nu = 1 - 2q_\nu \cos(\epsilon + u) + q_\nu^2, \quad \mathfrak{V}_\nu = 1 - 2\mathfrak{q}_\nu \cos(\epsilon - u) + \mathfrak{q}_\nu^2.$$

Denoting the thickness of the lamellæ by Δ, and putting

$$L = \frac{2\pi c_2 \cos(\epsilon + u)}{l_2} \cdot \Delta, \quad D = e^{-\frac{2\pi c_2 \sin(\epsilon + u)}{l_2}} \cdot \Delta,$$

there result, for the amplitudes a' and $a_{\prime\prime\prime}$, and for the retardations of phase d' and $d_{\prime\prime\prime}$, of the ray reflected in the first medium and of that refracted in the third medium, the following expressions :—

I. For light polarized parallel to the plane of incidence :

$$a'^2 = \frac{V_1 U_3 - 2D^2[((q_1^2-1)(q_3^2-1) + 4q_1 q_3 \sin^2(\epsilon+u))\cos 2L - 2(q_1 q_3+1)(q_1-q_3)\sin(\epsilon+u)\sin 2L] + D^4 U_1 V_3}{U_1 V_3 - 2D^2[((q_1^2-1)(q_3^2-1) - 4q_1 q_3 \sin^2(\epsilon+u))\cos 2L - 2(q_1 q_3-1)(q_1+q_3)\sin(\epsilon+u)\sin 2L] + D^4 V_1 V_3},$$

$$a_{\prime\prime\prime}^2 = \frac{U_1 V_3 - 2D^2[((q_1^2-1)(q_3^2-1) - 4q_1 q_3 \sin^2(\epsilon+u))\cos 2L - 2(q_1 q_3-1)(q_1+q_3)\sin(\epsilon+u)\sin 2L] + D^4 V_3}{16 D^2}$$

$$\tan d'' = \frac{-U_3 + 2D^2 \cot(\epsilon+u)[(q_3^2-1\sin 2L + 2q_3 \sin(\epsilon+u)\cos 2L] + D^4 V_3}{\frac{2q_1 \sin(\epsilon+u)}{(q_1^2-1)}}$$

$$-U_3 + 2D^2 \frac{q_1^2+1}{q_1^2-1} [(q_3^2-1)\cos 2L - 2q_3 \sin(\epsilon+u)\sin 2L] - D^4 V_3$$

$$\tan d_{\prime\prime\prime} = \frac{\sqrt{U_1 U_3} \sin(L-\psi) - D^2\sqrt{V_1 V_3} \sin(L-\phi)}{\sqrt{U_1 U_3} \cos(L-\psi) + D_2\sqrt{V_1 V_3} \cos(L-\phi)},$$

where

$$\tan\psi = \frac{(q_1 q_3 - 1)\sin(\epsilon+u)}{q_1+q_3+(q_1 q_3+1)\cos(\epsilon+u)}, \qquad \tan\phi = \frac{(q_1 q_3 - 1)\sin(\epsilon+u)}{q_1+q_3-(q_1 q_3+1)\cos(\epsilon+u)}.$$

II. For light polarized perpendicular to the plane of incidence:

$$a'^2 = \frac{\mathfrak{B}_1 \mathfrak{U}_3 - 2D^2[((q_1^2-1)(q_3^2-1) + 4q_1 q_3 \sin^2(\epsilon-u))\cos 2L - 2(q_1 q_3+1)(q_1-q_3)\sin(\epsilon-u)\sin 2L] + D^4 \mathfrak{U}_1 \mathfrak{B}_3}{\mathfrak{U}_1 \mathfrak{U}_3 - 2D^2[((q_1^2-1)(q_3^2-1) - 4q_1 q_3 \sin^2(\epsilon-u))\cos 2L - 2(q_1 q_3-1)(q_1+q_3)\sin(\epsilon-u)\sin 2L] + D^4 \mathfrak{B}_1 \mathfrak{B}_3},$$

$$a_{\prime\prime\prime}^2 = \frac{\mathfrak{U}_1 \mathfrak{U}_3 - 2D^2[((q_1^2-1)(q_3^2-1) - 4q_1 q_3 \sin^2(\epsilon-u))\cos 2L - 2(q_1 q_3-1)(q_1+q_3)\sin(\epsilon-u)\sin 2L] + D^4 \mathfrak{B}_1 \mathfrak{B}_3}{16 q_3^2 D^2}$$

$$\tan \delta' = \frac{+\mathfrak{U}_3 - 2D^2 \cot(\epsilon-u)[(q_3^2-1)\sin 2L + 2\mathfrak{q}_3 \sin(\epsilon-u)\cos 2L] - D^4 \mathfrak{B}_3}{\frac{2q_1 \sin(\epsilon-u)}{(q_1^2-1)}}$$

$$+\mathfrak{U}_3 - 2D^2 \frac{q_1^2+1}{q_1^2-1} [(q_3^2-1)\cos 2L - 2\mathfrak{z}_3 \sin(\epsilon-u)\sin 2L] + D^4 \mathfrak{B}_3$$

$$\tan \delta_{\prime\prime\prime} = \frac{\sqrt{\mathfrak{U}_1 \mathfrak{U}_3} \sin(L-\psi') - D^2\sqrt{\mathfrak{B}_1 \mathfrak{B}_3} \sin(L-\phi')}{\sqrt{\mathfrak{U}_1 \mathfrak{U}_3} \cos(L-\psi') + D^2\sqrt{\mathfrak{B}_1 \mathfrak{B}_3} \cos(L-\phi')},$$

where

$$\tan \psi' = \frac{(\mathfrak{q}_1\mathfrak{q}_3-1) \sin (\epsilon-u)}{\mathfrak{q}_1 + \mathfrak{q}_3 + (\mathfrak{q}_1\mathfrak{q}_3 + 1) \cos (\epsilon-u)},$$

$$\tan \phi' = \frac{(\mathfrak{q}_1\mathfrak{q}_3-1) \sin (\epsilon-u)}{\mathfrak{q}_1 + \mathfrak{q}_3 - (\mathfrak{q}_1\mathfrak{q}_3 + 1) \cos (\epsilon-u)}.$$

In order to exclude all uncertainty in respect of the quadrants in which the phases are to be taken, the tangent-formulæ are so written that their numerators and denominators always agree in sign with the sines and cosines. The amplitude of the incident ray is put equal to 1; and the remaining amplitudes, as well as the square roots occurring in the expressions for d_{III}, δ_{III}, are always to be taken according to their absolute positive values.

The formulæ-systems I. and II. differ only in this, that in the latter $\mathfrak{q}_\nu$ and $-u$ take the place of q_ν and u; consequently also $\mathfrak{U}_\nu$ and $\mathfrak{V}_\nu$ take the pace of U_ν and V_ν; while L and D preserve their values unchanged. Moreover the expression for $\mathfrak{a}_{III}{}^2$ contains in addition the factor $\dfrac{c_1^2}{c_3^2}$.

For *perpendicular* incidence,

$$s_1 = s_2 = s_3 = 0, \quad c_1 = c_2 = c_3 = 1, \quad u = 0, \quad q_\nu = \mathfrak{q}_\nu = \theta_\nu,$$

and the two systems become identical. It is true that, for reflected rays, a difference of phase $d'-\delta'=\pm\pi$ is indicated in the formulæ; but this proceeds solely from the *reversal of direction* of the ray. That is to say, if the æther-vibrations take place *perpendicular* to the plane of incidence (chosen as plane of coordinates), the same direction of vibration corresponds to the same phase of the incident and reflected ray; but if the vibrations take place *in* the plane of incidence, to the same phase corresponds the opposite direction of vibration of the normally reflected ray.

When $\theta_1 > \theta_3$, the formulæ remain valid for all values of the angle of incidence; then $q_1 = \infty$, $\mathfrak{q}_1 = 0$, and constantly

$$a'^2 = \mathfrak{a}'^2 = 1, \quad d' = \delta' = \pi.$$

If, on the contrary, $\theta_3 > \theta_1$, the formulæ hold good only as far as the limiting angle of total reflection at the third medium, for which angle $s_1 = \dfrac{\theta_1}{\theta_3}$, $s_3 = 1$, $q = \infty$, $\mathfrak{q}_3 = 0$. The case $s_3 > 1$ requires a special discussion.

For a vanishing thickness of the metal or for $\Delta = 0$, $\mathrm{L} = 0$, and $\mathrm{D} = 1$, the expressions for the intensity become Fresnel's formulæ of intensity; while the tangents of the phases become 0, as was to be expected, since in the derivation of system II. the coefficient of ellipticity was taken as $= 0$.

For $\Delta = \infty$, $D = 0$, we obtain

$$a'^2 = \frac{U_1}{V_1}, \quad a_{III}^2 = 0, \quad \tan d' = \frac{-2q_1 \sin(\epsilon + u)}{-(q_1^2 - 1)}, \quad \lim d_{III} = L - \psi,$$

$$\mathfrak{a}'^2 = \frac{\mathfrak{U}_1}{\mathfrak{V}_1}, \quad \mathfrak{a}_{III}^2 = 0, \quad \tan \delta' = \frac{+2\mathfrak{q}_1 \sin(\epsilon - u)}{+(\mathfrak{q}_1^2 - 1)}, \quad \lim \delta_{III} = L - \psi',$$

which are Cauchy's formulæ. The symbol lim. signifies that with increasing thickness d_{III} and δ_{III} approach the given limiting *form*. A constant limiting *value* is only present in the limiting case (to be hereafter discussed) $\epsilon = \frac{\pi}{2}$.

Systems I. and II. are essentially characterized by the peculiar connexion between the periodic and the exponential functions. The influence of the one or the other preponderates according to the value of the quantity $\epsilon + u$, which agrees with twice the value of the so-called " principal azimuth " of the elliptical polarization. That is to say, the value of the ratio

$$\frac{-\text{nat} \log D}{L} = \tan(\epsilon + u)$$

depends on this quantity. The angle ϵ may vary between the limits 0 and $\frac{\pi}{2}$.

1. The limiting case $\epsilon = 0$ corresponds to *transparent* media. In consequence of the relations subsisting between the quantities s_2, c_2, ϵ, u, then $u = 0$ or $u = \frac{\pi}{2}$, according to whether $s_2 < 1$ or $s_2 > 1$. The case $\epsilon = 0$, $u = 0$, $D = 1$, furnishes the known formulæ for ordinary reflection and refraction by transparent lamellæ, from which the exponential functions have vanished.

The case $\epsilon = 0$, $u = \frac{\pi}{2}$, from which follows $L = 0$, corresponds to reflection by a thin transparent lamella, for which the limiting angle has passed beyond the total reflection. Yet, as is known from the observations of Newton, Stokes, and Quincke, with sufficiently small values of Δ light can pass from the first into the third medium. In fact, according to the fundamental principles of Cauchy's theory of reflection, in a very thin layer on the other side of the bounding surface of the totally reflecting medium a motion of the æther takes place which is analogous to that in the superficial layer of metallic media, and can be regarded as a limiting case of it. By the insertion of the above values for ϵ and u, formulæ I. and II., if we put

$$q_1 = \tan \phi_1, \quad \mathfrak{q}_1 = \tan \psi_1,$$
$$q_3 = \tan \phi_3, \quad \mathfrak{q}_3 = \tan \psi_3,$$

become

$$a'^2 = \frac{(1-D^2)^2 + 4D^2 \sin^2 (\phi_1 - \phi_3)}{(1-D^2)^2 + 4D^2 \sin^2 (\phi_1 + \phi_3)},$$

$$\mathfrak{a}'^2 = \frac{(1-D^2)^2 + 4D^2 \sin^2 (\psi_1 - \psi_3)}{(1-D^2)^2 + 4D^2 \sin^2 (\psi_1 + \psi_3)},$$

$$a_{III}^2 = \frac{q_3}{q_1} (1 - a'^2) = \frac{16D^2 \cos^2 \phi_1 \sin^2 \phi_3}{(1-D^2)^2 + 4D^2 \sin^2 (\phi_1 + \phi_3)},$$

$$\mathfrak{a}_{III}^2 = \frac{\mathfrak{q}_3}{\mathfrak{q}_1} (1 - \mathfrak{a}_I^2) = \frac{c_1^2}{c_3^2} \cdot \frac{16D^2 \cos^2 \psi_1 \sin^2 \psi_3}{(1-D^2)^2 + 4D^2 \sin^2 (\psi_1 + \psi_3)},$$

$$\tan d' = \frac{-\sin 2\phi_1}{+\cos 2\phi_1} \cdot \frac{1 - D^4}{1 - 2D^2 \dfrac{\cos 2\phi_3}{\cos 2\phi_1} + D^4},$$

$$\tan \delta' = \frac{-\sin 2\psi_1}{-\cos 2\psi_1} \cdot \frac{1 - D^4}{1 - 2D^2 \dfrac{\cos 2\psi_3}{\cos 2\psi_1} + D^4},$$

$$\tan d_{III} = \frac{+\cos (\phi_1 + \phi_3)}{+\sin (\phi_1 + \phi_3)} \cdot \frac{1 - D^2}{1 + D^2},$$

$$\tan \delta_{III} = \frac{-\cos (\psi_1 + \psi_3)}{+\sin (\psi_1 + \psi_3)} \cdot \frac{1 - D^2}{1 + D^2}.$$

When the third medium is identical with the first, the formulæ are transformed into those given by Stokes[*] for this case. For $D = 0$,

$$a'^2 = \mathfrak{a}'^2 = 1, \quad a_{III}^2 = \mathfrak{a}_{III}^2 = 0,$$
$$d' = -2\phi_1, \quad \delta' = 2\psi_1 - \pi;$$

which, taking into consideration the signification of ϕ_1 and ψ_1, are the usual formulæ for total reflection.

2. The limiting case $\epsilon = \dfrac{\pi}{2}$, from which constantly follows $u = 0$, $L = 0$, is, apparently, not realized in nature. Nevertheless most metals approximate to it (especially the noble metals, which have hitherto been the easiest to obtain in transparent layers) much more than to the opposite limiting case $\epsilon = 0$. On the other

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[*] Trans. of Cambridge Phil. Soc. vol. viii. 1849.

hand, the result is remarkable in a theoretical point of view, that the system of formulæ corresponding to the case $\epsilon = \frac{\pi}{2}$ is almost perfectly identical in form with that above exhibited for total reflection by transparent lamellæ—namely, with the modification that the numerators of the expressions for $\tan \delta'$ and $\tan \delta_{\prime\prime\prime}$ receive the opposite signs. The condition $s_2 > 1$, which limits the variation of the angle of incidence, disappears in this case, and s_1 may increase from 0 to $\frac{\pi}{2}$, unless the greater values be excluded by the occurrence of total reflection at the *third* medium. When $\Delta = \infty$, we have the case of a total reflection for any angle of incidence. It is easy to prove that, with increasing thickness, neither in the refracted nor in the reflected light can there be maxima or minima of intensity; for the maxima of a'^2 and $\mathfrak{a}'^2$ would necessarily corespond to the minima of $a_{\prime\prime\prime}^2$ and $\mathfrak{a}_{\prime\prime\prime}^2$, and also to the maxima of the expression $\left(D - \dfrac{1}{D}\right)^2 + 4\sin^2(\phi_1 + \phi_3)$. But the second term in this is independent of the thickness; while the squared parenthesis possesses, between the limits $\Delta = 0$ and $\Delta = \infty$, no real minimum. Such a medium, therefore, in a layer of variable thickness, would not exhibit, either in the reflected or in the transmitted light, an interference-phenomenon analogous to Newton's colours of transparent bodies.

For the transmitted ray this assertion may be at once generalized thus far :—with increasing thickness maxima and minima of intensity can never occur, provided that $\epsilon + u > \dfrac{\pi}{4}$. For let us imagine both numerator and denominator of the general expression for $a_{\prime\prime\prime}^2$ divided by D^2; the numerator becomes independent of the thickness, and the denominator takes the form

$$N = U_1 U_3 D^{-2} + V_1 V_3 D^2 - 2(A \cos 2L + B \sin 2L),$$

where, as an easy calculation shows,

$$A^2 + B^2 = U_1 U_3 V_1 V_3.$$

The maxima, then, of $a_{\prime\prime\prime}^2$ must correspond to the minima of N, and *vice versâ*. Differentiation according to Δ gives :—

$$\frac{l_2}{4\pi c_2} \frac{\partial N}{\partial \Delta} = \sin(\epsilon + u)(U_1 U_3 D^{-2} - V_1 V_3 D^2)$$

$$+ 2\cos(\epsilon + u)(A \sin 2L - B \cos 2L),$$

$$\frac{l_2}{4\pi c_2}\frac{\partial^2 N}{\partial \Delta^2} = \sin^2(\epsilon+u)(U_1 U_3 D^{-2}+V_1 V_3 D^2)$$
$$+2\cos^2(\epsilon+u)(A\cos 2L+B\cos 2L).$$

For $\Delta=0$, $\dfrac{\partial N}{\partial \Delta}$ has the positive initial value

$$\frac{4\pi c_2}{l_2}.4q_1 q_3(q_1+q_3)\sin 2(\epsilon+u).$$

But the second differential quotient can never be negative for $\epsilon+u>\dfrac{\pi}{4}$, because we have

$$\left(\sqrt{(U_1 U_3)}.D^{-1}-\sqrt{(V_1 V_3)}.D^1\right)^2=U_1 U_3 D^{-2}+V_1 V_3 D^2$$
$$-2\sqrt{U_1 U_3 V_1 V_3})>0.$$

The numerical value of the parenthesis $A\cos 2L+B\cos 2L$ therefore cannot exceed the limiting values

$$\pm\sqrt{A^2+B^2}=\pm 2\sqrt{(U_1 U_3 V_1 V_3)}.$$

Therefore constantly

$$(U_1 U_3 D^{-2}+V_1 V_3 D^2)\pm 2(A\cos 2L+B\cos 2L)>0,$$

whence $\dfrac{\partial^2 N}{\partial \Delta^2}>0$ results for $\epsilon+u>\dfrac{\pi}{4}$. But hence it further follows that $\dfrac{\partial N}{\partial \Delta}$, starting from its positive initial value, constantly increases, and so can never become 0, and therefore that, in the light that passes through, there are no maxima or minima, but the intensity continuously diminishes as the thickness increases.

For the *reflected* ray, on the contrary, the condition for the maxima and minima assumes the form

$$U_3 D^{-2}\sin(2L+\epsilon+u-\phi)+V_3 D^2\sin(2L-\epsilon-u-\psi)$$
$$-2\sqrt{\frac{U_3 V_3}{U_1 V_1}}\sin 2(\epsilon+u)=0,$$

where

$$\tan\frac{\psi+\phi}{2}=\frac{q_3^2-1}{2q_3\sin(\epsilon+u)}, \quad \tan\frac{\psi-\phi}{2}=\frac{q_1^2-1}{q_1^2+1}\cot(\epsilon+u).$$

For greater values of Δ the influence of the term multiplied by D^{-2} becomes here evidently preponderant; and since this, in consequence of its periodic factor, infinitely often changes its sign, an infinite number of maxima and minima exist. These, however, diminish in distinctness very quickly, since the periodi-

cally fluctuating intensity very quickly converges towards the mean value $\dfrac{V_1}{U_1}$.

We have now to determine whether (speaking after the analogy of the phenomena of Newton's coloured rings), with a metallic layer increasing in thickness from the centre to the circumference, the middle appears bright or dark. For $\Delta = 0$,

$$\frac{\partial a'^2}{\partial \Delta} = \frac{4\pi c_2}{l_2} \cdot \frac{q_1 q_3^2 (q_1 - q_3) \sin 2(\epsilon + u)}{(q_1 + q_3)^3}.$$

This expression is positive or negative, according to whether the third medium is optically denser or rarer than the first. Thus, for example, with a metallic coating of this kind, of variable thickness, adhering to glass, the middle in reflected light must appear dark or bright, according to whether the reflection takes place in air or in glass. If the third medium is identical with the first, and so $q_1 = q_3$, then, for $\Delta = 0$, $a'^2 = 0$ and $\dfrac{\partial a'^2}{\partial \Delta} = 0$, and the middle appears dark.

The discussion of the expressions for $\mathfrak{a}'^2$ and $\mathfrak{a}_{\prime\prime\prime}^2$ leads to perfectly analogous results.

As regards the comparison of the given results with experiment, hitherto only some of the noble metals, particularly silver, gold, and platinum, have proved capable of being obtained in sufficiently thin, transparent layers. These metals are characterized generally by large values of the constant ϵ. For *silver*, when the reflection takes place in air, Jamin's observations give :—

	$\log \theta_1 =$	$\epsilon =$	$\theta_1 \cos \epsilon.$	$\theta_1 \sin \epsilon.$
For Fraunhofer's line D	0·4595	79° 15′	0·5373	2·830
„ „ H ′......	0·2740	77 16	0·4142	1·833

Quincke's series of observations (*Opt. Unters.* §§ 39–41) give the following values for the optical constants of thin lamellæ of silver and gold with reflection in air and in glass (these observations refer to red light):—

	Reflection in air.		Reflection in glass.		$\dfrac{\theta_1}{\theta_3}.$	$\mu.$
	$\log \theta_1.$	$\epsilon.$	$\log \theta_3.$	$\epsilon.$		
Silver on crown glass.	0·5742	87° 54′	0·3545	84° 4½′	1·658	1·5149
„ flint glass...	0·5349	86 24	0·3764	81 37	1·440	1·6258
Gold on crown glass.	0·4016	84 57	0·2985	82 43	1·268	1·5149

Here the observations contained in a horizontal series all refer to *the same* metallic layer. Accordingly theory would require that the values of the constant ϵ should be identical in both cases (reflection in air and reflection in glass), and that the quotient of $\dfrac{\theta_1}{\theta_3}$ should be equal to the exponent of refraction μ of glass, given in the last column. These requirements of theory are evidently only very imperfectly fulfilled—which necessitates the assumption that, in consequence of a different molecular constitution of the metallic surfaces adjacent to air and to glass, the constants of reflection assume somewhat different values. Similar are the results of the series of observations given in § 43, for the reflection-constants of the same silver surfaces in air, water, and oil of turpentine :—

$$\epsilon_1 = 87^\circ\ 24', \qquad \epsilon_2 = 87^\circ\ 52', \qquad \epsilon_3 = 86^\circ\ 9',$$

$$l\theta_1 = 0\cdot5178, \qquad l\theta_2 = 0\cdot4264, \qquad l\theta_3 = 0\cdot3595.$$

$$\frac{\theta_1}{\theta_2} = 1\cdot234, \quad \mu_2 = 1\cdot336, \ \left|\ \frac{\theta_1}{\theta_3} = 1\cdot440, \quad \mu_3 = 1\cdot474.\right.$$

With regard to *transparent* metallic layers the first question is, whether the degree of transparency required by theory agrees with that found by experiment. As the factor D^2 is contained in the expression for the intensity of the ray after transmission, that factor must first be determined. With the aid of the above given optical constants of silver for Fraunhofer's lines D and H, the values contained in the following Table, of $\dfrac{1}{D^2}$ for various thicknesses of silver, are easily calculated on the supposition of perpendicular incidence :—

Δ.	Fraunhofer's line		Δ.	Fraunhofer's line	
	D.	H.		D.	H.
millim.			millim.		
0·01	1·830	1·786	0·06	37·53	32·53
·02	3·348	3·192	·07	68·67	58·10
·03	6·126	5·702	·08	125·66	103·8
·04	11·21	10·19	·09	229·9	185·4
·05	20·51	18·20	·10	420·7	331·2

When, further, we set up the actual intensity-expression for the ray which has passed through the system of silver and flint glass, in which the loss of intensity in the passage through the uncoated surface of the glass also comes into consideration, we

find that the value of D^2 with perpendicular incidence is to be multiplied by a numerical factor which, as the thickness increases, approaches the limiting value 1·747 for Fraunhofer's line D, and 2·2556 for the line H ; so that the greater transparency for rays of higher refrangibility is thereby rendered more apparent.

Quincke found (*Opt. Unters.* § 52) that silver of 0·09 millim. thickness, gold of 0·16, and platinum of 0·40 still appeared transparent. The succession of degrees of transparency is, with regard to the optical constants of these metals, that which, from the theory, was to be expected; and the value 0·09 for silver may be regarded as a limiting value satisfactorily in accordance with the theory, since the extinction at this thickness has proceeded to about $\frac{1}{200}$ of the original intensity.

Although the phenomena hitherto discussed have revealed deviations from the theory which could only be explained by the assumption of certain molecular differences in the metallic surfaces, occasioning a variation in the optical constants, yet the general character of the phenomena was in accordance with the theory. On the contrary, a fundamental difference between theory and experiment occurs in the Newton's colour-rings observed by Quincke on thin metallic lamellæ (*Opt. Unters.* §§ 49–64). Silver, gold, and platinum undoubtedly belong to the group of metals for which the condition $\epsilon + u > \dfrac{\pi}{4}$ is fulfilled, for which therefore, as was above shown, in the transmitted light generally no maxima and minima can occur. As regards the maxima and minima of the reflected light, there are of course an infinite number of them, which yet, as may easily be shown, escape observation through their small intensity. Considering that, for 0·1 millim. thickness of silver, only variations of intensity of the order of about $\frac{1}{400}$ of the total can be expected, only the number of maxima and minima which as far as this thickness are, according to the theory, to be expected need be counted. For $\Delta = 0·1$ millim., $2L = \dfrac{4\pi c_2 \theta_1 \cos (\epsilon + u)}{\lambda_1} \cdot 0·1$ millim. In the simplest case of perpendicular incidence, $c_2 = 1$, $u = 0$; further, for Fraunhofer's line D, $\lambda_1 = 0·5888$ millim., so that we obtain

$$2L = 0·1825 \cdot 2\pi = 65° \ 42'.$$

As far as this thickness, therefore, the periodic terms of the function have not yet run through one fifth part of their period, and, independently of the bright or dark centre, in the favourable case perhaps only one maximum or one minimum will have been observed.

More remarkably, the breadth of the bright and dark streaks

observed by Quincke was not essentially influenced either by the colour or the angle of incidence. If, however, they are to be regarded as Newton's interference-lines, it will thence follow that Cauchy's theory of reflection cannot, without essential modification, be applied to such thin metallic lamellæ; considerably different values must be assigned to the optical constants; particularly the constant ϵ must have a considerably smaller value than for opaque metals, in order to be in accord with experiment. But thence will necessarily result the further consequence that, even in opaque metals, the arrangement of the æther in a superficial layer of measurable thickness is essentially different from that in the interior of the metal, so that the quantities l_2 and ϵ lose their character as constants, and that, as Quincke has assumed, the reflection takes place not in a geometrical bounding surface, but within an intermediate layer of finite thickness.

The middle of the system of fringes observed by Quincke, corresponding to $\Delta = 0$, appeared dark in silver lamellæ when the reflection took place in air, bright when in glass—which agrees with the above-developed result of the theory. The action of gold and platinum was somewhat anomalous.

I refrain for the present from a closer discussion of the expressions for the phases, and merely refer to an easily controllable result of experiment which likewise stands in contradiction to the theory. M. Quincke found (*Gött. Nachr.* Dec. 1870) that the difference between the directions of the rays transmitted through air and through metal amounted to nearly $\pm \pi$ for different thicknesses of metal $< 0 \cdot 04$ millim. For *minute* thicknesses of metal, the condition of a constant direction-difference between metal and air cannot generally be fulfilled by the theory. For greater thicknesses, the phase $d_{ııı}$ approximates, as before mentioned, to the limiting form $L - \psi$, and the quantity L, increasing with the thickness, would be equal to the corresponding quantity for air, if the direction-difference were to assume the constant value ψ. The condition thence following is

$$\theta_1 \cos (\epsilon + u) = \frac{c_1}{c_2}.$$

This cannot be fulfilled for any angle of incidence. Let it hold good for perpendicular incidence, and approximately for *small* angles of incidence, it will be reduced to $\theta_1 \cos \epsilon = 1$, whereas for silver $\theta_1 \cos \epsilon = 0 \cdot 5373$ was found above. But even in the case of its being fulfilled the constant direction-difference would not be found equal to $\pm \pi$, because the angle ψ always lies in the first quadrant.

Leignitz, January 1871.

LXII. *Intelligence and Miscellaneous Articles.*

ON HARMONIC RATIOS IN SPECTRA.   BY J. L. SORET.

THE idea of seeking harmonic relations between the different lines of the spectrum of one and the same body is doubtless not new, several savants having already occupied themselves with this question [*]; but the article which has just been analyzed [†], as well as Mr. Stoney's previous note, appears to us to present a special interest:—1st, by the extreme precision with which the calculated wave-lengths coincide with those deduced from experimental determinations which are capable of inspiring great confidence; 2ndly, by the high order of the harmonics which are indicated—such that the ratios of the numbers of vibrations are by no means very simple. The complication of these ratios, particularly for chlorochromic acid, as well as the absence of the greater number of the harmonics in the case of hydrogen, is even of a nature to excite doubts of the correctness of the hypothesis which is to be controlled.

Nevertheless the coincidence of the calculated with the observed values is too exact for it to be possible to attribute it to chance : if not due to the existence of harmonics, it must proceed from some other determinate cause. It seems, then, to us that here are motives for urging the study of this interesting subject. By taking into account the ultra-violet lines, of which a great number have already been determined photographically by M. Mascart, we should have a much more extensive field than if we confined ourselves to the visible spectrum, which does not comprise even an entire *octave*.

As an example, I will notice some relations at which I arrived with facility in a very superficial and incomplete examination of the question.

It is known that the spectrum of *magnesium* presents, among other bright lines, a group of three green lines (coinciding with the solar lines *b*). M. Mascart [‡], in studying the ultra-violet portion of this spectrum, has found two other groups of three lines perfectly resembling the preceding in their appearance; and to his record of this fact he adds :—" The reproduction of such a phenomenon can hardly result from chance; is it not natural to admit that these groups of similar lines are harmonics, depending on the molecular constitution of the luminous gas?"

The wave-length of the least-refrangible of the three lines in each

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[*] Among others, M. Lecocq de Boisbaudran (*Comptes Rendus de l'Acad. des Sciences*, 1869 and 1870) has noticed a great number of approximately simple ratios between the wave-lengths of the various lines belonging to one and the same body, as well as certain relations between the positions of the lines of different bodies

[†] Messrs. Johnstone Stoney and Reynolds's article, in the Philosophical Magazine for July 1871, " On the Absorption-spectrum of Chlorochromic Anhydride."

[‡] *Comptes Rendus de l'Acad. des Sciences*, 1869, vol. lxix. p. 337.

of these groups is given by the following numbers :—

<div style="text-align:center">

millim.

1st group, λ = 0·0005183 (Ångström's determination),
2nd ,, λ = 0·00038378 (Cornu's ,,),
3rd ,, λ = 0·0003335 (Mascart's ,,).

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Now the ratio between the first two numbers, $\dfrac{0·0005183}{0·00038378}$, is nearly identical with the ratio between the wave-lengths of the hydrogen-lines C and F, lines which Mr. Stoney regards as the 20th and 27th harmonics of one and the same fundamental vibration. The first two groups of magnesium also might therefore be regarded as the 20th and 27th harmonics of a fundamental group of vibrations, of which the wave-length, for the least-refrangible line, would be 0·0103660 millim. As to the third group, it would not represent the 32nd harmonic (as this is done by the hydrogen-line *h*), but, very nearly, the 31st.

Facts of the same kind are found also for the lines of *cadmium* which have been determined by M. Mascart *. Thus the ratio of the wave-length of the 1st line (λ = 0·00064370) to that of the 18th (λ = 0·00025742) is exactly as 5 to 2. Further, between the 2nd line of cadmium (λ = 0·0005377) and the 8th (λ = 0·00039856) the ratio $\dfrac{27}{20}$ is found, with an approach to accuracy very near the limit of the errors of observation. These two lines might, then, be regarded as the 20th and 27th harmonics of one and the same fundamental vibration. The 32nd harmonic is not found ; but, as in the case of magnesium, the 31st harmonic nearly coincides with the 10th line (λ = 0·00034645). The 6th line (λ = 0·00046765) represents very exactly the 23rd harmonic of the same fundamental. Finally, the 6th and 10th lines are also connected by the same ratio of $\dfrac{27}{20}$.

It seems difficult to admit these coincidences to be fortuitous ; and probably others still would be discovered on a closer examination of the question.—*Bibliothèque Universelle, Archives des Sciences Physiques et Naturelles*, September 15, 1871.

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ON THE ELECTROMOTIVE FORCE OF INDUCTION IN LIQUID CONDUCTORS. BY DR. L. HERMANN.

On the occasion of experiments on the excitation of the nerve by induction in itself, which I shall elsewhere communicate, the question suggested itself whether the electromotive force of the induction demonstrated by Faraday in liquid conductors was the same as that in metallic ones, other conditions being the same. Since under

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* *Annales de l'Ecole normale,* 1867, vol. iv. p. 28.

the latter the inducing force is independent of the nature of the metal, the question arises, does this independence of the nature of the induced conductor also extend to liquids, and therefore to all conductors? Experiments on this point have not been made, so far as I am aware.

I made three experiments with three different arrangements, of which I shall only communicate the most successful, as they all led to the same result.

In a metallic contact connected with the compass a piece of caoutchouc tubing 179 centims. in length was inserted; its external diameter was 13 millims., its internal 7 millims. In both ends amalgamated zinc cylinders were firmly tied; and the tubing was entirely filled with saturated solution of zinc vitriol. The intensities of the induced current were now to be compared according to whether the liquid part, or a corresponding one of the metallic circuit, or both simultaneously were exposed to inducing action.

A powerful Ruhmkorff's electromagnet was used as inductor, of the kind which serves for investigating the action on the plane of polarization. The current was furnished by two adjacent series, each of four Bunsen's elements. The two coils were provided with cylindrical poles of 65 millims. diameter and plane faces, and were so arranged that the two poles formed a cylinder between the spirals 55 millims. in length. About these the caoutchouc tubing was first of all coiled in six turns, then, in a further series of experiments, simultaneously with it a part of the metallic conduction, also in six turns, of approximately equal radius; the caoutchouc tube was finally removed and only the coils of wire left. The continuity of the induced circuit was unbroken during the whole of the experiments.

The compass was one of Wiedemann's, extremely sensitive, with the magnet made aperiodic by a sufficient degree of astatizing. The very slight inequality of the zinc electrodes in the tubing produced a slight deflection, which was almost constant during the entire experiment, which produces no disturbance, because the induced circuit is always closed. The electromagnet was removed as far from the compass as my laboratory permitted; it was in another room, at a distance in a straight line of 14·6 metres. The openings and closings were effected by my assistant, at bell signals given by an observer sitting at the telescope. The electromagnet was turned about the vertical until the action of its opening and closing was without any influence on the mirror of the compass when the circuit of the latter was open. The primary current was alternately passed through the electromagnet in the two directions, which I will call A and B.

The following are the deflections, exactly in the order in which they were obtained. Before Experiment 1 the electromagnet had been traversed by the current in the direction A.

I. Induction on the liquid Conductor (six turns).

Experiment No.	Direction of the primary current in the electro-magnet.	Induction deflection in parts of the scale.	
		On closing.	On opening.
1.	B	196*	88
2.	B	77	86
3.	B	86	86
4.	A	198*	88
5.	A	89	86
6.	A	89	85
7.	B	192*	86
8.	B	98	88
9.	B	90	86
10.	B	94	86
11.	A	203*	88
12.	A	92	87
13.	A	90	86
14.	A	86	84
Mean (excluding the deflections marked *)............		89·1	86·4

II. Simultaneous and like-directed Induction on six liquid and six metallic turns.

15.	B	375*	171
16.	B	173	168
17.	B	169	163
18.	A	381*	169
19.	A	171	168
20.	A	173	166
21.	A	176	166
		172·4	167·3

III. Simultaneous but contrary Induction on six liquid and six metallic turns.

22.	A	3	5
23.	A	4	6
24.	A	6	7
25.	B	26*	8
26.	B	8	7
27.	B	8·5	8
		5·9	6·8

IV. Induction on six metallic turns.

28.	B	85	83
29.	B	83	83
30.	B	84	84
31.	A	189*	84
32.	A	89	83
33.	A	88	83
		85·8	83·3

These experiments show with certainty, since the resistance in the induced circuit is in all cases the same, *that the electromotive force of the induced current is, in the widest sense of the word, entirely independent of the nature of the induced conductor.* The induction in Series I. and IV. is approximately equal, in Series II. approximately twice as great, and in Series III. approximately null. That these relations should be quite *exact* could not be expected, since induction takes place on other parts of the conduction besides the coils, and these parts change their positions somewhat during the arrangement of the experiments, and, lastly, the convolutions cannot be exactly limited to six complete turns.

After each alteration of the direction of the current in the electromagnet, the first closing regularly produces an uncommonly strong induction-current (the corresponding numbers are marked with * and are not taken into account in calculating the mean). This phenomenon doubtless has its origin in the fact that the electromagnet, after the opening of the current, retains a high degree of permanent magnetism ; so that in altering the direction of the current there is not only a magnetization, but also an inversion of the poles, which must, of course, produce a more powerful induction. Inasmuch as the latter amounts to more than double the ordinary one, it follows that the magnet on opening retains more than a third of its magnetism. Moreover it is observed that in by far the most numerous cases the induction-current on closing is a little stronger than that on opening, and that the intensity of the former is subject to somewhat stronger variations. This also can be readily explained : on opening, the magnet always loses the same quantity of magnetism ; this sudden decrease is followed by a further slower one. The increase of magnetism at the moment of closing will therefore always be somewhat greater than the decrease at the moment of opening, and will be greater the longer the pause between two experiments.

Exactly the same phenomena were observed in the two other series of experiments. In one the electromagnet was again used as inductor ; between its poles, however, an iron cylinder 149 millims. in length and 16 millims. in diameter was inserted. This was surrounded by a glass spiral of fourteen turns, width of coil 30 millims., filled with solution of zinc sulphate. In the other I used an inductor, to be described elsewhere, which I have constructed for nerve induction, and which is calculated for very short induced conductors ; in this case a simple straight glass tube or a metal wire was used for induction.—Poggendorff's *Annalen*, No. 4, 1871.

Zurich, April 1, 1871.

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END OF THE FORTY-SECOND VOLUME.

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